Contemporary Psychology

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Psychology's Genetic Half

Paul H. Mussen (Ed.)

Handbook of Research Methods in Child Development. New York: John Wiley & Sons, 1960. Pp. x + 1061. \$15.25.

Reviewed by ROBERT R. SEARS

Paul Mussen, selected by the Committee as editor, is Associate Professor of Psychology in the University of California at Berkeley and Research Associate in its Institute of Human Development. He was trained in clinical and social psychology at Stanford and Yale, taught at Wisconsin and Ohio State University, and has been at Berkeley since 1956. With John Conger he authored Child Development and Personality (Harper, 1956). He reviewed Suchman's laboratory manual of child development (Harcourt, Brace, 1959; CP. June 1960, 5, 202). The reviewer is Robert Sears, Professor of Psychology at Stanford University and Executive Head of its Department of Psychology. He is a Yale PhD, who became Director of the Iowa Child Welfare Research Station in 1942 and then of the Harvard Laboratory of Human Development in 1949. He has been at Stanford since 1953 where he busies himself with parent-child interaction and the identification of the child with his parents, He is CP's Consultant on developmental psychology. He reviewed David Levy's Behavioral Analysis (Charles C Thomas, 1958; CP, Oct. 1959, 4, 327f.).

This very solid thousand-page Hand-book was designed in the mid-1950s by the Committee on Child Development of the NAS-NRC, the four members of which were, and are, seasoned research veterans in child psychology. Alfred Baldwin, the Chairman, heads Cornell's Department of Child Development and Family Relationships; Roger Barker runs the famed behavioral ecology research at "Midwest" in Kansas; Boyd McCandless is Director of the Iowa Child Welfare Research Station: George Thompson is a professor of child psychology at Ohio State University. Paul Mussen, to whom the arduous job of editor was assigned, is a Yale-trained member of the faculty of psychology at Berkeley. A distinguished group, all in all, and one that speaks with the authority of current. productivity as well as past research. Their judgment as to what a committee could do that would be of greatest benefit to the field of developmental research must be taken most seriously.

What was most needed, they thought, was to bring together in a single source "concrete descriptions and evaluations of the most widely used research techniques in many aspects of the study of child growth and behavior." The focus

was to be on methodology; substantive content was to enter only for illustrative purposes. They wanted to explain the rationale of developmental study, to indicate by example the vast variety of research problems that exist for the behavioral scientist when he views behavior developmentally, and to show clearly and precisely what methods are available for solving these problems.

How successfully these ends have been achieved by the Handbook will be easier to determine ten years hence than now, for the proof of the pudding is not in the cooking. If developmental research shows improvement, if more psychologists begin to take a genetic point of view, perhaps this book can take some credit, though the mere fact that it could be written at this time suggests that the field is already yeastier than usual. There is good evidence for this belief in several of the chapters, for the effort to upgrade methodology also puts the theoretical substance of this branch of science on display; the do-it-vourself sign is bold and unmistakeable. It will be surprising if there are not some buyers. And that is a heart-warming thought, for child development has been undermanned and underworked for two decades.

METHODOLOGICAL handbooks have been few and far between in psychology, and no wonder. The heart of research competence lies in a peculiar form of intimate manipulative knowhow in the application of techniques to organisms. On the technique side, this requires an understanding of the unwritten and often unformulated principles that specify the boundary condi-

tions for its use, or the special sources of error that pervade it. On the organism side, it requires a deep and intuitive sensitivity to the special qualities that make that organism different from others, whether it be white rat, monkey, or human child. These things are hard to write about, because scientific writing is largely limited to those matters that have been directly investigated, Seven decades were required to formalize what Freud knew, from the beginning, about verbal reinforcement in psychotherapeutic interchange. But how do you communicate these intangibles in a handbook before they are clarified by direct experiment? Either you describe the external mechanics of the procedures (e.g., the method of paired comparisons or of parent interviewing) and thus satisfy the professional methodologists-but leave the beginner no further ahead than he would be with journal articles; or else you can try to convert the know-how to writing (e.g., the nature of set in a psychophysical experiment and how to control it, or the ways of making tape-recording acceptable to parents) in order to give the novice a leg up on his task-and earn the disregard of the profession's self-appointed superegos. Most of the authors of the present Handbook chose the former course, and hence there are mighty few relevant research techniques that have escaped formalized description. This is truly a handbook and probably the best in psychology since Whipple's time. It will be a very useful tool for the education of researchers in child psychology, but it will not be a substitute for apprenticeship, for the knowhow seeps through more rarely than one might wish.

The book's coverage of psychological areas of research is as broad as its coverage of individual techniques. The twenty-two chapters are ordered into five main parts. The first is devoted to general research methodology, the second to biological growth, the third to cognitive processes, the fourth to personality development, and the fifth to social behavior and the environment. This is a refreshingly different organization from that of the "physical, mental, emotional, and social growth"

classification that defined the field in the 1930s. There are still traces of the past, however, and the contemporary psychologist who has no familiarity with the history of Child Development (as a discipline, with its own society and journals) may be excused for wondering why a book containing 92 per cent (by volume) of psychology is labeled "development" rather than "psychology."

The reasons are historical. They are not important to an evaluation of this Handbook. Let it be noted, simply, that for what were good and sufficient reasons forty years ago, the human child became the defining reference point for a science. The growth of knowledge, the technical elaboration of methods, the construction of theory, have long since forced practicing scientists to specialize in the genetic aspects of one or at most two sciences (e.g., psychology, anthropology), but there is still a myth that we can have a sensible science organized around the child-relevant aspects of all the sciences. The Handbook bows to this myth, but the virility, mass, and complexity of the psychological methods it describes are likely to go far toward killing it.

Twenty of the twenty-two chapters are within the confines of psychology. broadly speaking. The tradition is responsible for the other two, one on physical growth and the second on chemical and physiological growth. The former is excellently done (by Howard V. Meredith) and is understandable to a psychologist, partly because it contains an admirable blend of formal description and less formal but highly valuable know-how. (It may have the effect of inducing psychologists and anthropologists to collect good data on growth.) The other chapter (by Icie G. Macy and Harriet J. Kelley) may well be of value to specialists in physiology and nutrition; it is narrow in scope and devoted to technical description at a level beyond the competence of most psychologists. This is no criticism of the chapter itself but only of the editorial decision that led to its inclusion. It is high time for child developmenters to face the fact that, while communication between scientists who have different methodological specialties is good,

the technical mastery of multiple research disciplines is virtually impossible. Let there be genetic *psychology*, genetic *biochemistry*, etc., but let there not be a pretense that the science is still so small in content and limited in its methods that there can be a single genetic science.

Another editorial decision—but one that deserves applause for the conception-was to provide a section on the methodology of developmental study as such. There are special principles that apply in this field and are not relevant to other branches of psychology. The skill in execution of the four chapters that compose this section does not match the wisdom of the plan, however, The first, by Alfred Baldwin, is a general commentary on what child development is, where it came from, what its ethical problems are, and what constitutes good theory. The level of discussion of this latter ranges from the elementary to the esoteric and is less effectively presented than other parts of the chapter. Most effective are the notes on ethical problems that arise in research on children. The second chapter, by William Kessen, is in two parts. The first is a highly verbalistic review of the logic inherent in the study of development. It starts with an analysis of just what the formula R = f(Age) means, continues through more sophisticated conceptions involving interaction influences, and culminates in a much too brief statement of the need for theoretical variables to account for the process of change. This is essentially a school piece on the logic of science and will be candy for those graduate students who make a career of criticizing research rather than doing it. The second part of the chapter is on problems of longitudinal research and the reasons therefor. It is less formalistic and correspondingly more useful, though again the message would be better communicated if there the atmosphere were more of the laboratory than of the library. Looked at as training materials, both these chapters will prove valuable for seminar discussions after a student has proved he knows what the dirty end of the research stick feels like.

The remaining two chapters in this introductory section deal with observational and experimental methods respectively. Presumably, they were intended to be parallel discussions of the two great classes of methods for securing variation in the independent variable, the naturalistic and the artificially controlled. Bijou and Baer's chapter on the experimental method turned out that way: it is a model of clarity, abstract enough to explain the general principles. of experimentation, but concrete enough to make the principles understandable in their application to child psychology. They use actual experiments well to illustrate relevant points as they arise. and a lot of the lore about boundary conditions they make explicit-when and why an experiment is better than other methods, and when it is not. They also discuss the implications of uncontrolled variation from biogenetic, life history, and concurrent sources with both a theoretical and a practical sophistica-

The chapter may prove to be more valuable for undergraduate study than graduate, for it suffers slightly from one defect that occurs more seriously in some other chapters, namely, the confusing of the problem with the method. Methods, techniques, instruments are constructed for a purpose. They are designed to measure a certain variable. The variable is relevant to some problem, or hypothesis. When a handbook author has to describe and illustrate a method, he is automatically limited to examples from completed studies. There may be a hundred new questions to be answered concerning the variable, and this method may be just what is needed for the new studies, but illustrations come from the past. Now, if the author gets a little confused as to whether he is talking about the method itself or the illustrations of it. he can easily fall into a manner of writing that sounds as if the method had already been used for everything it was good for, implying, indeed, that it properly should be used only for these researches that have already been done. When Bijou and Baer list five types of behavior for which the experimental method has been used in child psychology, they come precious close to

implying that these are the only kinds of problems for which experimentation is suitable.

This confusion is much more severe, however, in the other basic methodological chapter, by Herbert Wright on observational techniques, a chapter that is less successful than Bijou and Baer's for the purpose it must have had, namely, of delineating the use of naturalistic, as contrasted with experimental, data. This is just too bad, because that problem has an important place in a handbook of research on children. Two topics seem, however, to have got mixed



PAUL H. MUSSEN

up in this one chapter. What came out on top was the description of observational techniques, that is to say, research methods in which a trained human observer, rather than a test or a brass instrument, is the measuring instrument. But even for this subject matter, the chapter is disappointing because the author's zeal for the research problems relating to behavioral ecology got the better of him and led him to some methodological evaluations on quite irrelevant grounds.

Wright classified observational techniques into six types. This classification is sound enough, but one unstated assumption creeps into the evaluations to destroy much of their cogency, the assumption that all observation is performed for the purpose of securing as complete a phenomenological description of events as possible. The discussion almost makes it seem that the ultimate criterion of the effectiveness of an observation method would have to be the similarity of its output to that of a sound motion picture. This criterion may be valuable for ecological research, but it is nonsense for many other kinds. In scientific

investigation, measurement of events is aiways done for a purpose. Research is performed to answer some question that has been posed on the basis of a theory, or because of a need for normative data, or because of simple exploratory curiosity about the relationship between two variables. The human observer is for these purposes no different from any other measuring instrument. Just as one might wish to study the effect of pain stimulation on respiratory rate, so he might wish to study the effect of the dependency supplications of younger children on the frequency and kind of nurturant responses of older ones. A pneumograph with polygraph would be most suitable for the first study, and a properly trained human observer for the second. In neither case would there be any point to measuring variables other than those relevant to the questions being asked in the investigation.

The consequence of the author's point of view in this chapter is that some crucial methodological points have been ignored. For example, time sampling has received rather brief and unsympathetic treatment, and hence such crucial matters as the relation of time samples to behavior unit observation, the techniques and difficulties of securing observer reliability, the theoretical relation of time units to behavior strength, and the differences between trait consistency and observer reliability, have been largely ignored. Since the use of human observers as measuring instruments for complex behavior variables (i.e., for something other than dial or marker reading) is the only truly unique methodological contribution of child psychology to general psychology, it would seem appropriate for the editors to have ensured a more balanced coverage of it.

THE remaining four parts of the Handbook are devoted to detailed reporting of methods and techniques that have been used for specific classes of variables. Part II, on biological growth, contains three chapters, two of which have been mentioned earlier. The third, on receptor functions, by Austin Riesen, could as easily have been included in Part III, The Cognitive Processes, Riesen has presented a beautifully clear description of the psychophysical methods, and a careful discussion of the specific problems (e.g., attention span, mo-

tor skill, understanding of instructions) that arise in their use with children. His illustrative examples are particularly effective in displaying the ways in which children's sensory capacities can be estimated. Both this chapter and the next, on experimental studies of perception, by Eleanor Gibson and Vivian Olum, gain greatly from their authors' success in separating method from problem, and indicating briefly some of the unsolved problems for which the methods will prove useful. In this connection, one might guess that the influence of the various parts of this book will be noticeably related to the imaginativeness the various authors have shown in their hints for the direction of future research. By this criterion. I predict that the next few years will see a greater increase in doctoral dissertations on the genetics of receptor functions, perception, language, and psycholinguistics than on children's learning, problem solving, or ability testing.

In the two language chapters, Orvis Irwin and the team of Jean Berko and Roger Brown have done quite different types of analyses for obvious reasons. Both have high merit. Irwin's description of phonetic recording has a solid down-to-earthness that makes clear both how and why. Berko and Brown, faced with a field of linguistics that as yet has little to offer on method, made the best of a tough assignment by embedding that little in a brilliantly lucid summary of the field itself; any psychologist looking for greener research pastures will be strongly tempted to move into Brown's. In contrast, Spiker on learning and Anastasi on ability testing offer little to excite the newcomer. The learning chapter suffers from the method-cum-problem confusion, and the ability chapter contributes nothing to an understanding of how to deal with the testing of children. If there is any virtue at all in the child development point of view, it is the emphasis on the unique characteristics of children as research subjects. Yerkes' contribution to biological psychology was considerable because he published and published on the unique qualities of chimpanzees; Harlow knows the same sort of thing about monkeys, and Skinner about pigeons. A lot of child psychologists know

it about children, but neither Spiker's nor Anastasi's chapters would lead one to suspect so. If there is one thing a student might hope to get from a methodological handbook on child development, it is the know-how, the lore, the special problems (and their solutions) in the use of scientific techniques with children.

The first chapter of Part IV purveys this wisdom in abundance. Leon Yarrow's discussion of interviewing children is focusing almost entirely on what can and cannot be done with them through language and the play technique. Furthermore, he goes into the reasons why. Any thoughtful study of the chapter will provide a liberal education on what children are like and what it is like to do research with them.

THE remainder of Part IV consists of four other chapters on methods of measuring children's personality, and at this point editorial control evidently abandoned an impossible battle. The dimensions of personality study are too ill defined to permit the careful assignment of separate areas that obtained in the previous sections. Projective measures, nursery school or playground observations, teacher rating-scales, and many other devices enter all the chapters in some degree. If there is redundancy and repetition, however, the several authors' analyses are sufficiently different to make the mélange richly rewarding to a potential researcher looking for wisdom in this helter-skelter field. Technically, I suppose. Part IV is badly edited; practically, this is its saving grace. Neither authors nor editor can be blamed for the state of the field. and, while these presentations do not add much order to it, they do provide a thorough coverage.

Part V, which moves over to dyadics, small groups, families, and whole cultures, is equally rich and, by the nature of its material it is better organized. All four chapters are of high order with respect to the infúsion of theory as well as the reporting of methods. It is a satisfaction to find William Lambert finally making a point bluntly that should have been made nearly a thousand pages earlier: the methods you use depend on the theory you hold and the problems you attack.

In an otherwise tremendously effective volume, this evaded issue is the one source of discomfort. Child psychology as of 1960 is incredibly more sophisticated with respect to both theory and problems than was the general psychology of Titchener's day, or Whipple's, The latter's Manual presumed only a simple notion that there were a lot of mental functions, and any test that was reliable was a useful measure of one of them. With that level of theory, a handbook was easy to build. Contrast that happy summertime of psychology's childhood with the complicated miseries of its young maturity. Even the notion of reliability itself has become so involved in the theory of behavior that for vast areas of study (personality development, for example), reliability is no longer a meaningful concept.

So we may conclude that the original decision to leave out substantives except for the illustration of method was wrong. Most of the authors knew their way around in the field well enough to sneak some of it in anyway. For those of us who enjoy including a little chauvinism in our allegiance to the genetic dimension of metapsychology, the Handbook is a splendid gift from the Committee, the Editor, and the authors, one for which we can express honest gratitude. It demonstrates once and for all that the genetic approach must properly be contrasted with the point of view of action-interaction, and that, except for the structural, there is no other. This Handbook, in other words, covers almost one-half of all psychology.



Organization is by now a part of every man, but inquiry is not. The significant product of science and education will be the incorporation within the human animal of the capability and habit of inquiry.

Logic for the Clinician

Theodore R. Sarbin, Ronald Taft, and Daniel E. Bailey

Clinical Inference and Cognitive Theory. New York: Holt, Rinehart and Winston, 1960. Pp. x + 293, \$5.50.

Reviewed by PAUL E. MEEHL

There are three authors. Sarbin is Professor of Psychology at the University of California in Berkeley. He published a methodological analysis of clinical prediction in 1944 and made the first systematic empirical research comparing clinical and actuarial methods in 1941. Taft is a Reader at the University of Western Australia. He has been concerned with the ability to judge others. He and Sarbin together published An Essay on Inference in the Psychological Sciences (Garden Library Press. 1952). Bailey is Research Associate at the University of California in Berkeley. With Block he has developed an actuarially-based set of Q-sort descriptions appropriate for MMPI interpretation. The reviewer, Paul Meehl, now President of the American Psychological Association, is known to everyone. He is Professor of Psychology at the University of Minnesota and Professor of Clinical Psychology at its Medical School. He calls himself a "hybrid clinician and rat-psychologist," but of course he is well known for his contributions in the logic of science and on psychological models. He is a Diplomate in Clinical Psychology and has served on the American Board of Examiners in Professional Psychology, the first psychologist to serve on that Board who was not a 'Grandfather' Diplomate.

THIS book formulates all clinical inference in terms of a general theory of cognition, based mainly upon the ideas of Brunswik and Tolman and bringing together research from clinical, social, perceptual, and learning psychology, but its ambitious aim to provide an adequate reconstruction has not been achieved because the authors' analysis

suffers from four fundamental and pervasive errors.

First is their confusion of psychological and logical questions. Although aware of Reichenbach's distinction between the contexts of discovery and justification (pp. 77, 80, 84), they rarely make it, leaving the reader unclear as to when he is being offered causal analysis and when logical reconstruction. Such metalinguistic terms as postulate, inference, syllogism, and premise are applied to nonlinguistic events, a departure in "usage from that of the classical logician" (p. 46) which is deliberate. "Inference is the cognitive transformation of one set of events through another set. . . . This does not imply that the process . . . is accessible to self-report" (p. 45). "Some of the propositions . . . cannot [!] be expressed in sentences" (p. 46). The use of another discipline's technical terms in a way expressly forbidden within that discipline requires a very strong defense, which is not provided. By such odd semantics they dispose of clinical intuition and creative hypothesis-formation, incorrectly attributing to Allport and Meehl the preposterous notion that such events "emerge from the void without knowable antecedents" (p. 82) or are "without a history or evolution" (p. 187)-views specifically rejected in the very monograph they are discussing (See P. E. Meehl, Clinical versus Statistical Prediction, Univ. Minn. Press, 1954, pp. 51, 53, 58f., 82),

Secondly, all valid inference is reduced to the categorical syllogism, a restriction harmonious with the authors' preference for "taxonomic" clinical examples. "All fulminating schizophrenics are dangerous; this man is a fulminat-

ing schizophrenic; therefore, this man is dangerous," p. 52). This is not the kind of 'hard case' to examine when analyzing clinical inference! Structural-dynamic hypotheses involving construct variables rather than behavioral predicates are avoided. "The equivalence of other syllogistic forms to the categorical can be demonstrated" (p. 52) is true in a sense far too abstract to justify their taxonomic approach.

Thirdly, empirical knowledge is grossly oversimplified by identifying all non-deductive inference with simple enumerative "induction" [= frequency-counts]. This is the book's core mistake, which rationalizes its simplistic treatment of the clinician as knower. For example, clinicians are said to validate a hypothesized father-identification by determining whether the "length of the list of similarities exceeds some arbitrary value" (p. 233). I assume that other psychotherapists will find this 'frequency' model of corroboration as ludicrous as I do. All nonsyllogistic inference except enumerative "induction" is labeled "noninductive" and considered fallacious. I am not here raising a 'philosophical' issue to criticize a substantive 'psychological' theory; the book is consciously epistemological and its cognitive model is defended by philosophical arguments, mostly unsound.

A large group of clinical inferences (including most of the 'hard cases' for a purely actuarial view) are called "fallacious" because they "affirm the consequent" (pp. 233, 234, 235, 266). Are the authors really unaware that all empirical inference is in the third figure of the hypothetical syllogism and hence (formally) "invalid"? This is precisely what differentiates formal and empirical sciences, producing the statisticians' controversies over Fisher and the logicians' puzzles about confirmation. The form: p[nomothetic theory or idiographic hypothesis] entails q[experimental or naturalistic observation]; q[observation made]; 'therefore' p[hypothesis corroborated], is the standard model of empirical inference in science, history, the law courts, and common life. This 'therefore' is obviously not intended as the ergo of deduction. If the authors' syllogistic criterion were generally thus misapplied, all human knowledge (except pure logic and mathematics) would be discarded as "fallacious." Whatever one's views on inductive logic, no reconstruction can afford to classify the third figure as illegitimate in the empirical domain. Even the logician Karl Popper, who disbelieves in any special inductive logic (e.g., Carnap's 'degree of confirmation'), readily allows the third figure as corroborative.

Fourthly, the authors assume that all theoretical constructs are susceptible of a purely abstractive analysis, other types being condemned as "fictions" (pp. 232, 233, 236). To denigrate clinicians as "not always rational" (p. 72), their constructions as "arbitrary" or "whimsical" (p. 233), and third-figure corroducing a "glow experience" (p. 235), on the ground that such hypothesizing does not fit an *epistemological* model which is itself considered unsound by most logicians, is pretty high-handed!

In clinical settings, it is not uncommon for a behavior analyst to instantiate an occurrence and collocate the resulting minor premise with a major premise containing a fictional entity. For example, the occurrence about Jones, "talks excessively," provides the minor premise: "Jones is a member of a species characterized by excessive talk." If this is collocated with the major: "all persons who talk excessively are orally fixated," then the inference follows that Jones is orally fixated. But oral fixation is a dispositional construct, a fiction. How can the behavior analyst determine if the statement has truth-value? If the fictional entity appears to be congruent with expectations in the general postulate-system of the inferrer, then the inferrer may regard his inference as having validity. In this instance, if the analyst believes that all humans may be characterized by placement on a dimension of psychosexual maturity, and if orality is measured by any oral manifestation that is intense or frequent, then the conclusion is regarded as possessing intrinsic validity.

A moment's reflection reveals the fallacy in this chain of reasoning. The occurrence which was specified as the operational measurement of the construct has no necessary relationship to the construct. "Excessive talking" may be related to an infinite number of fictive antecedents. To borrow from a medieval theory, the major premise might be, "A person who talks excessively is possessed by demons." There-

fore, Jones is possessed by demons. (P. 232.)

This tendentious passage is typical of the book's anti-psychoanalytic bias (how is it that a treatise on clinical inference indexes Meehl's name 19 times. Cronbach's 8, Estes' 5, but Freud's only twice?) The inference from "talking excessively" to "orally fixated" is not deductive, nor would any competent psychoanalyst assert the "major premise." Furthermore, if "fiction" is defined epistemologically (i.e., as a construction by human minds trying to make causal sense of observations), then fictions are not necessarily 'bad,' although a particular fiction may be rejected on scientific grounds. Caloric no longer appears in the nomological network of physics, having been replaced by the fiction of molecular motion. If, on the other hand, the term fiction is meant scientifically (e.g., Don Quixote is 'fictional,' because no such person existed), then the construct "oral fixation" must be examined on its merits. The authors easily settle a difficult empirical question (Is there an entity = oral fixation?) by invoking an irrelevant epistemological truism (Construct-terms are not in the observation-language).

The possibility of differential empirical tests is rejected:

In order to eliminate such alternate constructs, the clinician may seek confirmation of his conclusion by noting other instances which are presumably related to the construct. To pursue the same illustration: Jones has been observed picking his teeth. "Picking one's teeth" is regarded as another exemplar of oral fixation. The conclusion then "affirms" the first conclusion: Jones is orally fixated.

The persistence of the employment of congruent validity is no doubt influenced by the fact that clinicians create and use one set of conjectures and not another. Suppose the same minor premise had been collocated with a major premise containing a different construct: persons characterized by cleanliness are anally fixated; Jones picks his teeth (a form of cleanliness—dental hygiene); therefore Jones is anally fixated.

In short, defining an occurrence term as an instance of a dispositional construct is arbitrary and may even be whimsical. It is simply not possible to attach an empirical truth value to clinical inferences in which the predicate is fictive. (Pp. 232f.)

Thus the mere existence of multiple hypotheses capable of explaining the same facts is taken to demonstrate the illegitimacy of hypothesis-construction, as if science and inductive logic lacked methods of differential corroboration leading to rational choice.

The nondeducibility of explanatory hypotheses from their corroborating explananda is *not* a matter on which tastes may differ or 'philosophies of science' conflict but is a matter of formal logic and just about as controversial as the binomial theorem. When we say that explanatory hypotheses are only "suggested" by the facts, we are not only making a psychological comment in the context of discovery; we express also a *logical* relation in the context of justification, namely, that the entailment is only one-way.

RESTRICTING their clinical examples to the taxonomic and dispositional becomes difficult for these authors when the three "strategies of search for input" (scanning, scrutinizing, and probing) are analyzed (pp. 154-159). In this otherwise insightful discussion, theoretical constructs are spoken of as if somehow peculiarly observable. "In probing, the perceptual apparatus is directed toward occurrences that are not on the surface but must be uncovered in order to make an inference" (p. 156). The clinical "inferrer . . . removes emitted but irrelevant occurrences [a neat trick!] from the ecological surface which may conceal relevant events on the subsurface" (p. 157). The ecological surface is "more finely inspected" and "broken" (p. 175) by suitable interview tactics. Such locutions, which convey an impression that interviewers peel off outer layers of a personality to 'see' what's underneath, are necessitated by the poverty-stricken logical apparatus. Adopting this picture would retard development of mathematical and structural models suitable for formulating and testing psychodynamic theory. One does not "inspect" unconscious behavior determinants by removing some sort of obfuscating shell; he hypothesizes them. We must insist upon this methodological point regardless of our views on current psychodynamic theory, because the latter can be neither corroborated nor disconfirmed without the invention of models which, while more rigorous and explicit than present verbal formulations, possess a structural richness adequate to represent the essential substance.

The authors also avoid historical reconstructions and hypotheses involving complex content in exemplifying the model. A case can be made that personology is more akin to history than to either botany or physics, a view with which the book never really comes to grips, thereby neglecting a rich source of counter-actuarial support. Personologists like Allport, Holt, McArthur, and Murray cannot be expected to accept even the few 'historical' examples analyzed, since those chosen do not reflect the distinctively historical content and method of clinical reconstructions. "Severe child-rearing practices produce neurosis . . . early toilet-training is one of those practices . . . a late toilettrained person will be free from neurosis" (p. 123). Perhaps the clinician's process of historical reconstruction can be analyzed into components of this kind; but such a claim must be tested upon the complicated instances clinicians adduce.

The authors' attitude 'toward historical and documentary method is revealed in the following passage:

The idiographic method—the truth-value of which depends upon internal consistency—is the method of history, biography, and literature. In these enterprises inferences need have no future reference. Logical coherence (congruent validity) is the test of the truth-value of a proposition. But when inferences have a future referent—when decisions are likely to follow from inferences—then prediction becomes the pragmatic test of truth. The novelist or biographer is justified in using internal consistency as a criterion; we require something more from the behavior scientist. (P. 256.)

Here again, a difficult methodological issue is settled by fiat. Is the time relation between a hypothesis' invention and observing one of its implications relevant to the degree of corroboration? Logicians disagree. Simon thinks it is, Carnap thinks not, Feigl is undecided. How can the authors make such a de-

batable point a test of who is a "behavior scientist"?

The stimulating chapter on "modules" as the basic cognitive unit which has a stochastic isomorphism with the "ecology" might provide tools sufficient for analyzing structural-dynamic hypotheses. but instead the purely taxonomic and dispositional orientation prevails, so nothing interesting happens. The reductionistic bias of the authors appears even in the actuarial context, where they quickly dismiss configural scoring (p. 242) without understanding it. The essential feature of configural prediction systems as originally defined by Meehl and rigorously generalized by Horst, Lubin, and others is the nonvanishing of secondorder mixed partial derivatives (corresponding to interaction effects in the discontinuous case). Allegedly, such systems can be handled by linear regression through population subdivision by a cut on the configurated variables' difference distribution. That is, a configural function F(x,y) = ax + bxy + cy is represented by two linear functions $f_{\mathbb{R}}(x,y)$ $=k_xx+k_yy$ and $f_m(x,y)=m_xx+m_yy$. the decision to apply f_k or f_m being determined by an inequality $(x - y) \ge d$. This implies that two sets of N_k and N_m simultaneous linear equations in two unknowns always have solutions, which is of course false.

All in all the book is very disappointing. The authors have surveyed and summarized the relevant research, but they integrate it by means of an inadequate logical model. Whatever plausibility the resulting conceptualization has is achieved by ignoring the really interesting cases of personological inference. As a move in the history of ideas about clinical cognition, we must regretfully rate the volume as essentially retrogressive.

Ш

One has merely to declare one's self free, and one feels the moment to be conditioned. But if one has the courage to declare one's self conditioned, then one has the feeling of being free.

-J. W. GOETHE

The Begging of Animals

Karl H. Winkelsträter

Das Betteln der Zoo-Tiere. Bern (Marktgasse 9): Verlag Hans Huber, 1960. Pp. 92. DM 14.80.

Reviewed by ETHEL TOBACH

The author, Dr. Winkelsträter, was born in Berlin and studied at the Sorbonne, where he first became interested in natural history, at Munich, Tübingen, Zurich, and the museum of natural history in Saarbrücken. At Zurich he became involved in the study of the comparative behavior of men and animals, and took his doctorate with a dissertation under Heine Hediger in comparative behavior and animal ecology. Hediger is the author of The Psychology and Behavior of Captive Animals in Zoos and Circuses (Criterion, 1957; CP. June 1957, 2, 160f.). The reviewer. Dr. Tobach, has been since 1957 Research Fellow in the Museum of Natural History, after her PhD at New York University. She works on stress and tension in animals, especially the rat, but also the mouse, along with parturition in the cat and gravidity in the rabbit.

This short theoretical essay views the begging of animals in zoos, circuses, national wild-life preserves, and homes as a learned "anthropogenous" behavioral pattern, which is considered to be firmly based upon innate needs and activities. The author's formulations are the result of some six years' observation in various European zoos and circuses. Throughout the book, abstracts of protocols are inserted as examples of particular analyses. In addition, there are some appropriately illustrative photographs and sketches.

Begging is defined as an activity which assumes a direct dyadic relationship with an opposite, a relationship between an entreater and a giver, one which arises out of an inner need toward a particular object which is not readily attainable. Such an act is to be .distinguished from the 'instinctive' begging during the interaction of parents and young or during mating, and from the mere expectancy of feeding. The complex pattern may result either from passive accumulation of knowledge or from active and purposeful 'trial-andsuccess' learning. It may also become an example of the analysis of new purposeful situations which may be used to accomplish the animal's aim. As an example, the author gives a protocol in which an Indian elephant placed small stones on a rock which could be easily reached by her trunk that she might "symbolically show the way" to visitors who had been unsuccessful in throwing food into her more distant enclosure. No control observations or experimental procedures are described, however.

The author emphasizes the affective component of begging. In an abbreviated description of an experiment utilizing an 'actograph,' changes in activity during feeding-expectancy and begging are shown graphically for a ferret and a rabbit. His point that these changes evidence the emotional characteristic of begging is difficult to see in the data presented. The increase in activity is patent; the interpretation needs validation.

Begging may be a "misguided occupation" (Hediger) and an indication of stress, as the author frequently illustrates. At other times it may be adaptive. The author found that, during begging, three primates would accept nine types of food which could be arranged in an ascending preferential order. As the visiting day wore on, the animals would become more discriminating in the kinds of food that they accepted. By the end of the day, they ate only the specially preferred foodstuffs. By prefeeding them with the more desirable foods (candy, bananas, salty foods), this hierarchy of preference was overcome for a period lasting as long as 60 hours. Animals were also found to beg for the foodstuffs that were lacking in their diets. The author interprets these instances as examples of the "principle of dynamic self-regulation" (Katz). One is reminded here of C. P. Richter's work on the selfregulatory activity of animals.

In view of the paucity of material dealing with this subject, it would have been useful to have a fuller presentation of the data. This interpretive survey nevertheless adds to some previous discussion on begging by Hediger and develops the investigation of the phenomenon in depth by placing it in the context of complex learned behavioral patterns.

Psychology Vis-à-Vis Speech Disorders

Dominick A. Barbara (Ed.)

Psychological and Psychiatric Aspects of Speech and Hearing. Springfield, Ill.: Charles C Thomas, 1960. Pp. xii + 756. \$10.50.

Robert F. Hejna

Speech Disorders and Nondirective Therapy: Client-Centered Counseling and Play Therapy. New York: Ronald Press, 1960. Pp. viii + 334, \$6.50.

Albert T. Murphy and Ruth M. Fitzsimons

Stuttering and Personality Dynamics: Play Therapy, Projective Therapy, and Counseling. New York: Ronald Press, 1960. Pp. viii + 519. 86.50.

Reviewed by Joseph M. Wepman

Barbara is an MD psychoanalyst associated with the American Institute of Psychoanalysis and Head of the Speech Department of the Karen Horney Clinic in New York City. He is author of The Art of Listening and of Your Speech Reveals Your Personality (both Charles C Thomas). Hejna is Director of the Speech and Hearing Clinic of the University of Connecticut and Professor of Speech there. Murphy is Co-Director of Boston University's Speech and Hearing Center, and Miss Fitzsimons is Head Speech and Hearing Therapist in the Public Schools of Warwick, Rhode Island. Wepman, the reviewer, is Director of the Speech and Language Clinic at the University of Chicago and Professor of Surgery and Psychology. He is a Diplomate in Clinical Psychology and author of Recovery from Aphasia (Ronald, 1951). He reviewed Wendell Johnson's Onset of Stuttering (Univ. Minn. Press, 1959; CP. 1959, 4. 394).

ALTHOUGH these three books are quite dissimilar in content, they have, nevertheless, a common core of being written about speech problems of different types and appear to be written for speech clinicians. This commonality tempts the reviewer to treat them as a single book. The authors, by the selection of their topics and their implied audience, have faced a dilemma which is difficult to resolve and which each in its own way has failed to resolve. The field of speech correction is by its own tradition a 'symptom-centered' field. Psychology, both in theory and practice, is 'person-centered.' The one studies and works with speech disorders almost ignoring the person involved; the other seeks to explain, to understand, and to work with the person, with his disorders seen as expressions of his maladaptiveness. Any rapprochement between the two fields because of their disparate emphasis is not only difficult, but almost impossible in the manner attempted by these books. The dilemma posed here is apparent in the very titles

of the books being reviewed-speech and hearing do not have psychological or psychiatric aspects, people do, and most of them also have speech and hearing. Nondirective therapy is no different for children with speech disorders than for any other children, for by its very nature, as one author (Hejna) indicates, the speech disorder is not 'reflected' by the clinician; rather the attitude of the client is his concern. Stuttering is a behavioral manifestation in man, who also has certain idiosyncratic personality psychodynamics (expressed at times through his speech). The stutterer, in a manner of speaking, has both stuttering and a dynamic structure; so too does every other person, and stuttering per se is comparable to many other aspects of his behavior. It is as though one were to attempt to place shoes and apples in the same distribution because both are useful to man. It can be done but how meaningful the relationship will be is highly questionable.

All three of the books appear to be undecided as to their audiences. By and large they are too sophisticated for the novice and too elementary for the knowledgeable. Most clinical psychologists will find nothing new in them, whereas the untrained speech clinician will find so much that is new that very little will be meaningful to him. In general, these books will prove useful for reference or for adjunct readings in the more intensive courses in psychology and psychotherapy.

THE Barbara collection by some twenty-five authors suffers from the weakness of all such multi-authored compilations. It is very uneven. Some of the chapters are exceedingly good. challenging in concept and direct in statement. I was impressed by the chapter on the need for speech clinicians' understanding themselves before they attempt to understand their patients. (An old idea in psychology and psychiatry, but a new one in the field of speech and audiology.) The discussion of the concept of stuttering as a neurosis has been stated before, yet it bears repeating. These chapters are well done in the present volume. The recognition that speech clinicians should shift

away from the 'learning-centered' approaches to the 'psyche-centered' seems a proper and worthwhile concept for the speech clinician. On the other hand, the chapters dealing with classical psychoanalytical interpretations of body parts, such as the ear, seem to be of a different order of conceptualization. A whole mass of background will be necessary before such conjectures can be meaningful to the readers. It is questionable whether, within such a volume, a single empirical study of articulation should have been included; it is so evidently a doctoral-dissertation type of exposition and not at the level of the other chapters. One reader commented that the book approached many new subjects for the beginner in such a way as to make the ideas a lure for further reading and that the style of most of the presentations was much like an annotated bibliography with the suggested readings that must be read before the subject matter can be understood.

The Hejna book on nondirective counseling and play therapy follows, although at considerable distance, what might be considered the classical Rogerian approach to writing about clientcentered therapy. Its pages are replete with the verbatim of interchange between therapist and clients. There is little reference to speech defect except in the book's title. Perhaps that is just as well. The book has little depth. It provides no theory as to why this approach should be useful with speech disorders and it offers little evidence that its approach does in fact help that kind of behavior in the clients. In defense of the book it should be said. however, that too little has been written about play therapy with children. By its tone the book indicates that this author has been successful in what he has attempted to do with children's disordered speech. It would have been better had the book dealt more with interaction between the therapist and the client and less with the verbal constructs of the technique employed. Too often phrases like "reflection of feeling," "consistent limits," "nondirectiveness." "passivity," and "patience" are used to explain behavior while the meaning of these terms, as well as the integrated action underlying them, is left to the assumption of the reader. For the trained counselor the volume will seem very old hat as though neither Rogers, to whom the book is dedicated, nor the myriad of counselors who have studied and written of client-centered therapy had changed since the early exposition of this approach in 1945. For the counselor the book will appear dated, for the novice it will be too sketchy.

HE third volume, by Murphy and Fitzsimons, on stuttering and personality dynamics, is one of a long series of such books. It essays the task of demonstrating that stuttering is after all a psychogenic problem and that stutterers need to be understood as having disturbances of the psyche rather than a speech problem. Granting that for the most part nothing new has been added by this book, still one may note that it is a better than average presentation of this point of view. The book deals quite adequately with the personal qualities of the clinician. The reader is left, however, to infer what the training of the clinician has been. The speech clinician, as defined by the book, is also a trained clinical psychologist, as so very few are in reality. The book presents, in very condensed form, the theory and practice of psychodynamics and psychotherapy. It leaves the reader a bit breathless, as though he were reading a condensation of almost everything he had read for the past twenty-five years. My fear would be that it would lead to somewhat superficial knowledge for the beginning student, unless he already has a broad lexicon of psychological literature behind him; but then, he would not be a beginning student and he would find this formulation dull.

There is much that is new and persuasive in each of the three books. They deal with what is known to be a problem which is not easy to resolve, and all of the authors should be commended for their effort. The goal is worth while. While their shortcomings are clear the books do constitute pioneering attempts to expand the horizon of the speech clinician into the broader world of psychology.

The Process of Mass Communication

Joseph T. Klapper

The Effects of Mass Communication. Glencoe, Ill.: Free Press, 1960. Pp. xviii + 302. \$5.00.

Reviewed by LOTTE BAILYN

The author, Dr. Joseph Klapper, is associated with the Behavioral Research Service of the General Electric Company where he conducts research on communication. In 1954 he prepared a monograph on Children and Television for Columbia University's Bureau of Applied Social Research. The reviewer, Dr. Lotte Bailyn, with a 1956 PhD from Radcliffe College, is Research Associate in the Laboratory of Social Relations at Harvard University. She wrote a monograph in 1959 on Mass Media and Children (Psychol. Monogr., 1961, 73).

The author introduces his book with a discussion of the need for a "clearing house" of writings, published and unpublished, that bear on the question of the effects of mass media. His point, and hence in part the justification for this book, is well taken. The number of publications on this topic is staggering, and continuity in research demands that predecessors be taken into account. It is a major virtue of this book that it provides an effective entrance into this mass of material.

The book-"an analysis of research on the effectiveness and limitations of mass media in influencing the opinions, values, and behavior of their audiences" -is divided into two parts: the effects of persuasive communication, and the effects of specific types of media material. The first part deals mainly with "such short term opinion and attitude effects as are typically the goals of campaigns-political, civic or institutional." This section is organized according to what Klapper calls the "direction" of effects: the creation of opinions or attitudes, reinforcement or diminution of the intensity of already existing opinions, and conversion to a view opposite to the one previously held. The second part is a less unified series of chapters on the effect of violence and of escapist material, the effect of adult television on children, and the relation between media consumption and passivity.

In these areas Mr. Klapper surveyed all "published reports of disciplined social research"—over "1000 studies, essays, and reports"—"more than 270" of which represented valid and new information or ideas and are specifically included in the book. The result is a comprehensive review of the literature, covering both research and speculation.

MR. KLAPPER'S aim in writing the book reaches, however, beyond the presentation of an obviously valuable summary of writings. He has attempted to synthesize the material by a unified interpretation. The larger question he is seeking to answer is how to understand the process by which mass communications affect their audience.

His answer comes in the form of five generalizations which he has published before and presents here again, particularly in the introduction and in the sections on theoretical considerations, generalizations which he reevaluates in his concluding note. Briefly put, Mr. Klapper's view is that only seldom do mass communications produce direct effects; most often they function "among and through a nexus of mediating factors and influences" which tend to reinforce existing conditions. Only if these reinforcing factors are inoperative or are themselves impelling to change, will the mass media contribute to a change in behavior, attitudes and values, or in the opinions of their audience. These mediating factors are external to the content or source of the communication. They consist of such elements as selectivity in exposure, perception, and retention, "inter-personal dissemination of mass communication," personal influence and opinion leadership, and group norms.

To Klapper, therefore, the process of effects is a process of mediation, but to this reviewer such a view does not seem to be a description of "process." It seems, rather, to be only a necessary first step, for it merely characterizes a set of factors which must be taken into account in analyzing this process. The concept of "process," however, though not readily defined, surely involves more than this. At the very least this set of factors must be ordered into a temporal sequence in which the fact of exposure has an assigned place. Further, one would want the steps in this sequence to be defined in such a way that the links between them are readily understandable-at least intuitively, if not in terms of established empirical correlations or accepted psychological mechanisms.

Even if Klapper's generalizations are not accepted as a description of the process of effects, nevertheless they still are useful, for they provide the author with an organizational scheme, one that is especially effective in the first part of the book, and they also lead him to suggest interesting new research problems. More importantly, they reinforce the value of what he calls the "phenomenistic" approach to mass communications research, an approach that "begins with the existing phenomenon-an observed change of opinion, for example-and which attempts to assess the roles of the several influences which produced it." Subject to the author's own cautions, this approach, as opposed to one that begins with only one influencemass communication-and searches for effects, is certainly a sensible one. And its corollary, frequently emphasized by Klapper, that socially relevant research must do more than define patterns of effect-it must inquire also into the relative incidence of the conditions underlying these patterns-seems particularly important.



DISSENT

THERE is a sense in which partial achievement is better than complete success, a sense in which the unattainable is the best goal. Human beings and other living organisms are adaptive systems. Their unremitting effort is directed toward an adaptation which is never perfect because the constantly changing environment in which the system operates requires a perpetual compensatory adaptation. You would think that an organism might sometimes achieve complete adaptation to rest complacent with no adaptive activity necessary, but we know that such a psychophysiological nirvana would turn out to be failure and not success. Man and rat seek activity; for either of them complete undisturbed adaptation would end in neurosis or psychosis. Sensory deprivation does not come to an organism as success, and release from responsibility is not what man or rat really needs. It is for this reason that increased leisure is not a proper goal for Labor.

Long ago Richard Avenarius (1888) expressed this paradox in his conception of the life processes as vital series of changes set up by the perpetual imbalance of the organism between the catabolic environmental forces and the anabolic internal adaptive forces. The reason that this paradox is obvious and not preposterous is that life is a series of events within an ever-changing system, and activity—change adjusted to other change—is the essential and indeed the criterion of life.

A similar paradox occurs in the intellectual life. One gets ahead in the kind of thinking that advances civilization by being forever remotivated by frustration. We dislike frustration but we need it. We need it in order to avoid it, and our dislike of it is useful in that it pro-

vides the motive to avoid it. This is indeed how science goes on forever, how every success in research poses more new problems than could be seen in the original undertaking, how knowledge never comes anywhere near abolishing ignorance. An investigator welcomes and uses new problems like a rat in an activity cage. Science and scholarship constitute a perpetual struggle to advance wisdom, yet how horrible it would be were knowledge to become complete, were life-giving ignorance to be abolished!

It is such a view of intellectual avidity that suggests why dissent plays so crucial a catalyzing role in creative thinking. Dissent is a generator of energizing frustration. It is the occasion for renewed activity, the interjection that protects a thinker from the deprivation inherent in terminal success.

And it is here that you may perceive the explanation for one of CP's dominating motives during these first six years of its existence. CP has welcomed the frustration of dissent as a stimulus to the intellectual avidity that it has been striving to promote. CP-not without some bitter criticism-has been as much concerned in the dynamic undertaking of stimulating thought as in the duller service function of purveying information. That is why CP is so much less disturbed by disagreement within its pages than are the dissidents themselves. In science truth is not absolute -so CP keeps insisting-and CP would feel that it had accomplished a worthier mission if it had tortured its readers with a healthy contribution of indecision than if it drugged them with a full measure of the incontrovertible.

To this philosophy many of *CP*'s readers will offer vigorous dissent. Good! The generation of dissent is part of *CP*'s mission. It is from frustration that civilization emerges.

It has been fifty years now since the editor of CP was a student of Titchener's in the Cornell Laboratory, that oasis in America where honor and protocol were esteemed so highly. There a senior man would often formulate a problem, a junior would execute it in the laboratory, and then the senior would write it up, sometimes without even consulting the junior, and publish it—always with the senior author's name last, because that was noblesse oblige: the honor of primacy was given to the junior, for the senior, being secure, could not stoop to competition with his junior.

The rule extended to other relations. There were, for instance, two papers which this editor published with his Chief. The editor, clearly the junior, had the idea, worked it out, wrote the paper, but added Titchener's name because the publication was clearly from the Laboratory. Of course Titchener, as senior, put his name second, for so, it was supposed, the junior got the greater credit

Nowadays there is a convention about putting the principal author's name first. It is a rule warped by equivocality—as when the authors think they have made equal contributions and may presently reverse this order in a second paper, hoping to average out to justice. If the principal author is also the senior, then his name comes first, and that may be all right, although older men may miss the graciousness of noblesse oblige. These matters must, nevertheless, work themselves out as the Geist changes.

Let us not, however-so says CP1 in one of these last moments before it stops complaining-let us not confuse senior with principal. "E. G. Boring and E. B. Titchener" read the by-line for each of those two papers. Would it ever occur to anyone to call Boring the "senior author" in that dyad? Could the "L.G.A." of Cornell in those years ever have possibly been designated "junior"? No. and we should not try to wrench this word away from its proper meaning. One author can be chronologically senior to another, or academically senior, or perhaps, if the difference be very obvious, senior in

prestige, but surely he cannot be senior in originality or diligence, nor in any of the attributes in respect of which his alleged seniority would fluctuate from time to time as his participation varies in a common enterprise. Senior has a clear meaning. Let us keep it to add stability in an uncertain, ever-changing world, for age is irreversible and seniority for this reason stays put to contribute its drop of information to the student who keeps wondering as he reads.

Just the same CP ought not to confuse its readers in these days when nobility has become so antiquated as seldom to allow the dignity of generosity to obscure the accuracies of relative prestiges. Heaven knows how often CP, speaking to modern success-culture, has misled its readers, but here are two accuracies that it neglected and can now put right. Reiff and Scheerer wrote Memory and Hypnotic Age Regression (International Universities Press, 1959; CP, Mar. 1961, 6, 70-72) and CP said correctly that Martin Scheerer is the senior author but neglected to add that Robert Reiff is the principal author and thus had his name placed first. Semmes, Weinstein, Ghent, and Teuber wrote Somatosensory Changes after Penetrating Brain Wounds in Man (Harvard Univ. Press, 1961; CP, Aug. 1961, 6, 272f.), carefully arranging the names in that order to show that Josephine Semmes is the principal author, and CP began by saying that Hans-Lukas Teuber is the senior. He is, sure enough, but CP should not have emphasized his antiquity, for he was only 87 days old when his future principal author appeared on the mundane scene, and everybody knows that girls mature more rapidly than boys-are perhaps born more mature. By the way, neither Reiff nor Semmes is mad at CP. This semantic problem, as it applied in these two cases, got itself raised adventitiously. and the answer formulated itself in a couple of discussions which can best be described as good, clean fun.

CP's WOMEN

Last June *CP* was remarking that, while about one-third of American psychologists are women, only about one-twelfth of *CP*'s book reviewers are women (*CP*, 1961, **6**, 205). *CP* sug-

gested that this disproportion arises because women psychologists find employment in areas where writing for publication is not so important as it is in psychology at large, where it contributes less to professional success. That was a 1959 sample of reviewers.

Now Dorothy Ransom comes forward with the complete tables for the first five years of *CP* and the sex ratios for both the reviewers and the authors of books. Here are her data.

Reviewers (each time, either as sole or as joint reviewer):

Yr.	M	F'	%F
1956	228	21	8%
1957	177	20	10%
1958	150	17	10%
1959	188	20	10%
1960	180	16	8%

Book authors (each time, either as sole or as joint author):

F
%
%
%
%
%

Book authors (each time, only when principal or sole author):

Yr.	M	F	%F
1956	216	18	8%
1957	185	21	10%
1958	164	18	10%
1959	187	27	13%
1960	187	25	12%

So in five years it took 1576 reviewers to review the work of 1017 authors if one counts separately joint authorship of books and reviews and separately repeated authorship; 9.2% of the book authors were women and 12.1% of the reviewers. These data bring the women up a few percentage points in scriptorial activity but do not change the general implication that women psychologists in America write less for publication in proportion to their numbers than do the men. And why shouldn't they? If psychologists did nothing but communicate, what in the world would they find to say? There needs to be division of labor and perhaps biology influences the division-that is to say, within psychology. Everyone knows that, in general, authorship is not foreign to the verbal sex.

NEXT spring Lippincott expects to publish Robert I. Watson's Great Psychologists, accounts of the contributions of 57 significant men in the history of psychology. The chief jewels on this string are Aristotle, then a 700year break, Augustine, a 1000-year break, Aquinas, a 400-year break. Descartes, Locke, Berkeley, a 150-year break, Fechner, Helmholtz, Wundt, Galton, James, John Watson, Wertheimer, Freud. Bob Watson then adds to this string the sketches of 43 other important psychologists, those related to the first-named fourteen, as introducing them or showing their consequences. or else as related to one another: Thales, Plato, Galen; Hobbes, Hume, the Mills: Johannes Müller; Brentano, Külpe, Bain; Comte, Charcot. Ribot; Titchener, Cattell, Hall; Pavlov; Köhler; and then twenty-five more to make up the fifty-seven varieties. Lotze, Hering, Stumpf, and Kraepelin just miss out. as do McDougall, Thorndike, and Lewin. Galileo, Francis Bacon, and Darwin are great enough but not quite psychological enough. They did not make it. Thus history is formed, like the Constitution of the United States, by the judgments of successive generations of wise men. The publication of the book will help to signal the growth of interest in the history of psychology, which is now clear in America as a notable trend of the present decade. CP thinks this is a sign of approaching maturity for psychology, for it to become concerned about its past. What you learn from history is how science and scientists respond to situations, what kinds of events and attitudes help progress and what hinder. Watson's book itself should be one of the helps.

In America the psychologists talk about Piaget, accept him as a great man, and do not understand him—largely because he works and writes from within an intellectual milieu that is strange to the predominant modes of thought in American psychology. Now it seems, however, that Dr. John H. Flavell of the University of Rochester, a clinical psychologist trained at Clark University, is about to publish, with the

aid of Van Nostrand, a book that tells in English all about what Piaget is thinking and doing—seven chapters on theory, three on Piaget's experiments, two on evaluating what others are doing. The title might be The Developmental Psychology of Jean Piaget. Roger Bibace of the Worcester State Hospital in Massachusetts, a man who knows Piaget and his work and who has reviewed Peter Wolff's monograph, The Developmental Psychologies of Jean Piaget and Psychologies of Jean Piaget and Psychologies (International Universities Press, 1960; CP,

Oct. 1961, **6**, 351f.) says that Flavell's chapters are very good indeed, and that American psychologists will benefit greatly by the excellence of the account and the familiarity of English.

Wolfe Mays, Professor of Philosophy at the University of Manchester and at one time a collaborator of Piaget's, also has a book coming along that deals with the more epistemological aspects of Piaget's thought, but *CP* has less advance information about this volume.

-E. G. B.



JUM C. NUNNALLY, JR.

Mental Health: What Kind of Information?

Jum C. Nunnally, Jr.

Popular Conceptions of Mental Health: Their Development and Change. New York: Holt, Rinehart and Winston, 1961, Pp. viii + 311, \$5.00.

Reviewed by M. Brewster Smith

The author, Jum Nunnally, had his PhD from the University of Chicago when Leon Thurstone was still there, was then at the Institute of Communications Research at the University of Illinois, where he came into contact with Charles Osgood and, of course, the Semantic Differential, and is now Professor of Psychology at Vanderbilt University. He is the author of Tests and Measurements: Assessment and Prediction (McGraw-Hill, 1959), and for CP reviewed Gurin, Veroff, and Feld, Americans View their Mental Health (Basic Books, 1960; CP, Aug. 1961, 6, 263f.). The reviewer, Brewster Smith, is Professor of Psychology at the University of California in Berkeley. He is the retiring editor of the Journal of Abnormal and Social Psychology, recently president of the Society for the Psychological Study of Social Issues, and currently Vice-President of the Joint Commission on Mental Illness and Health, that has just issued its final report, Action for Mental Health (Basic Books, 1961; CP. Sept. 1961, 6, 289ff.). This relatively slim but data-packed book, the product of a five-year research program at the University of Illinois, directed initially by Wilbur Schramm and later by Charles Osgood and the author, will properly interest three potential audiences: the growing number of persons involved in mental health education together with others more generally concerned with the problems of mental health; students of persuasive communication and attitude change; and-since the technique is relied on so heavily-psychologists who are interested in applications of Osgood's Semantic Differential. For the first of these audiences, the book is compulsory reading, for it reports one of the three major sets of data that begin to provide empirical bearings in an area that has been characterized more by good intentions than by clear goals, established principles, or ascertainable results.

These studies follow a very different strategy from the other two bodies of relevant research—Americans View their Mental Health, recently published by Gurin, Veroff, and Feld of the Michi-

gan Survey Research Center, and the still unpublished study directed by Shirley Star for the National Opinion Research Center. The latter are based on extensive interview surveys of representative national samples, conducted and analyzed in the tradition of survey research. In contrast, Nunnally and his collaborators diversify their investment over a variegated set of studies, each much less expensive than a national survey. Their studies include mail questionnaires to "mental health experts" and to a panel of respondents in central Illinois, interviews with respondents in the Champaign-Urbana area, content analyses of rather narrowly based samples of mass media, informal interviews with decision-makers in the mass media, and experimental studies of change in information and attitudes in captive audiences of high-school students. Each of these many studies is 'minor' in comparison with the investment called for by a national survey, none is 'definitive,' but the ensemble provide a range of information, coarsely textured though it is, that amply justifies the strategic decision. Though some of Nunnally's conclusions will be challenged, criticism will not center on the generally intelligent compromises made in the selection of the samples.

A second respect in which the Illinois studies differ from the national surveys in the field of mental health is seen in the kinds of data they bring to view: profiles for information factors, scale and factor scores on the Semantic Differential, counts for content categories —in a word, scores; means, standard deviations, and correlation coefficients rather than the percentages, 'breakdowns,' and free-answer quotations that emerge from survey analysis. The emphasis on quantification has obvious advantages, but there are costs, too, in the use of instruments and analytic techniques that so deliberately restrict the range of the information caught in the investigator's net. From this perspective, it is the Michigan and Chicago interview surveys that are more broadly exploratory.

These differences in approach contribute to giving the Illinois report an entirely different 'personality' from Americans View their Mental Health. Where the survey analysts compose an integrated, if sometimes conjectural, story from their less structured, more malleable materials, Nunnally binds together a series of discrete though related research reports, in which the data. presented less selectively than is customary with survey research, are left to a much larger extent to speak for themselves. The result is lucidly presented and interesting to read, but it seems in this respect more like a set of research memoranda than a book.

The major findings may be grouped around Nunnally's distinction between information—that is to say, beliefs about mental illness—and attitudes toward the mentally ill. toward their treatment, and toward the various 'mental-health professions.'

Information is measured by agreement-ratings of 180 opinion-statements culled from the literature on mental health, from intensive interviews, and from the mass media, and factor-analyzed to yield ten factor scores. Comparison of the responses of a public sample with those of 'experts'-ABEPP psychologists and GAP psychiatristsshows that by this criterion the general public is not seriously misinformed, although the older and poorly educated segments of the population do diverge considerably from the experts. Converging evidence indicates rather that people are relatively uninformed about mental illness, insecure about the information that they possess, and hungry for more, although "their interest is

largely restricted to learning ways to meet or avoid threatening situations" (p. 236). People want authoritative answers and will "gobble up any seemingly factual and authoritative-sounding information"—but they seldom get the closure they want from the experts.

As for attitudes toward the mentally ill, the studies using the Semantic Differential showed them to be grossly unfavorable. Nunnally concludes:

The widespread 'bad' attitudes are held not because of existing information, but rather because of the lack of it. Early in our research, we hypothesized that to relieve the threat associated with mentalhealth problems the important ingredient is for people to think that they have a valid system of information regardless of its real validity. Our research results tended to bear out this hypothesis. We found that the mere act of changing from one set of opinions to another . . . promoted favorable attitudes toward the mentally ill and toward treatment specialists and methods. This was so even though the new 'information' was in many cases less correct than what had been believed initially (p. 237).

In toto our studies show that the factual content of messages is important largely to the extent that it induces a proper emotional state. A message will promote favorable attitudes toward mental-health concepts if (1) the concepts are visible in the message . . . or associated with visible concepts . . . , (2) the message has a high interest value, (3) the message is thought to come from an authoritative source and (4) the message makes the reader feel secure by sounding certain, by providing solutions, by presenting an understandable explanation, and by reducing anxiety in other ways. If these content characteristics are present, people will develop more favorable attitudes and will be more open to continued learning about mental-health phenomena. If these characteristics are not present, no amount of sermonizing, haranguing, or factual presentation will work; and it would be better not to communicate at all (p. 238).

Before mental health educators take this passage as a charter for rushing in with solemnly authoritative pronouncements where scientists fear to tread, a pause for reconsideration is in order. Provocative and informative as Nunnally's results undoubtedly are, the fact that the group at Illinois put all their eggs in the basket of the Semantic Dif-

ferential, where attitudes are concerned, seems to me unfortunate. Remember that the Semantic Differential consists of a set of 7-point bipolar scales, such as foolish . . . wise, sad . . . happy. passive . . . active, unpredictable . . . predictable, dirty . . . clean, which respondents are required to apply to the 'concepts' under investigation. Mean ratings on 16 such scales are given in an appendix for 74 'concepts'-ones like insane man, neurotic woman, mental patient, but also a person who is sick with jealousy, someone who lives in constant fear that people are after him, and for comparative purposes, child, a leper, me, etc. As the fourth and fifth examples indicate, it is possible to phrase concepts that sketch in capsule form various sorts of mental disorder. But the concepts employed in the experimental studies of attitude change seem more often to have been like the first three examples, and the question arises in any case as to the psychological status of the 'attitudes' that are being tapped. If the level is one of verbal stereotypes or 'brand names, changes in evaluation may be less consequential than appears at first blush.

This view would seem to be supported by the finding that applying different labels to the same description of a case markedly shifts its semantic profile. Verbal stereotypes do make a difference, and it is important for a communicator to know that much of the vocabulary of talk about mental health is loaded with negative connotations. Where attitude change is at issue, however, one wishes, all the same, that enough variety of measures had been employed to have permitted some discrimination between changes in more deep-seated attitudes and also ones at the more superficial or narrowly verbal

In appraising Nunnally's recommendations, moreover, questions of objectives have to be raised that the book never considers. What attitudes do we want to change, and why? What information do we want to communicate? Among other things, we presumably want people to be more accepting toward former mental patients; we want to break down the barriers that isolate the large mental hospital from the urban community; we want people to support more generous and adequate care for mental patients, and more research on cause and cure; we want people to seek proper treatment for themselves and for members of their families at the early point when we think it would help most; we wish (but do not know enough to advise with the confidence that Nunnally requires of us) that people would comport themselves in ways more conducive to mental health in themselves and in their children and associates. The connotative meanings of the specimen concepts investigated by Nunnally's group would seem pretty remote from the attitudinal objectives involved in these purposes.

We also wish (at least we should) that people could be helped to become less arbitrarily dependent on the 'expert,' more adept in the handling of causal information that would make them more independently competent in managing their own lives. The confident expert pronouncement may allay anxiety and promote positive 'attitudes.' but surely it does not advance this objective. Even Nunnally's evidence that well-spaced but contradictory messages, confidently put, leave 'attitudes' no worse off than in the beginning, is hardly reassuring: is it really desirable to allay the communicator's anxiety about adding to the present cacaphony of 'expert'

Yet Nunnally is surely right about the immensity of the barriers to effective public communication that occur in an area as charged with anxiety as this one. Add to the foregoing considerations a point that he side-steps-the relative lack of factually based consensus among experts on just the practical and personal aspects of mental health in which public interest is keenest-and this reviewer can hardly escape the conclusion, in Nunnally's words, that in general "it would be better not to communicate at all" via the mass media. Nunnally's experimental evidence supports skepticism about the effectiveness of much current exhortation in the field of mental health. Still further skepticism, it seems to me, is in order concerning what would be accomplished by the sort of well-engineered. Madison-Avenue treatment of mental health education that one could easily imagine following from attempts to apply the Nunnally findings. 'Expert' support for such a skeptical view may be found in the recent (1960) report of the National Assembly on Mental Health Education (Pennsylvania Mental Health, Inc.: Mental Health Education: A Critique).

If, one the other hand, mental-health educators focus on interactive personal communication with specially motivated audiences, including 'gatekeeper' groups like teachers and pediatricians, the nature of communication becomes different from what Nunnally studied, more

ambitious goals for it become appropriate, and quite different criteria of effectiveness are required.

In spite of these misgivings about the implications Nunnally draws from the Illinois findings, let it be said that the research adds considerably to our knowledge of the popular conceptions of mental health and contributes significantly to the study of attitude change in an emotionally charged area. All three audiences specified at the beginning of this review should profit from a close but critical reading of the book.

Facts and Values in Adjustment Behavior

James C. Coleman

Personality Dynamics and Effective Behavior. (With selected readings prepared by Alvin Marks.) Chicago: Scott, Foresman, 1960. Pp. 566.

Reviewed by George H. Frank

The author, Dr. Coleman, is Associate Professor of Psychology at the University of California at Los Angeles and Director of the Clinical School there. His PhD is from the same institution. He has taught clinical psychology at the Universities of Kansas and New Mexico. He is the author of Abnormal Psychology and Modern Life (Scott, Foresman, 1956) and, with F. B. Libaw and W. D. Martinson, of Success in College (Scott, Foresman, 1960). The reviewer, Dr. Frank, is Clinical Assistant Professor of Psychology in the University of Miami at Coral Gables. His PhD is from Florida State University, and his clinical wisdom has been gathered at the Florida State Hospital and the Menninger Foundation.

A LTHOUGH the volume under review may be classified with books for a course in mental hygiene or personal adjustment, it is unique as such. Some such texts devote considerable space to conditions of psychopathology: how to recognize it, avoid it, and what to do should one fall prey to the demons of

neurosis or psychosis. Here, not more than one chapter is devoted to Faulty Patterns of Adjustment, a chapter that reviews the gamut of personality difficulties. Few approaches to mental hygiene or personal adjustment are written from the point of view of what Marie Jahoda called the positive concept of mental health, but this book is. It assumes that adjustment involves more than avoidance of psychopathological conditions. Here one notes the strong influence of the theoretical conceptualizations of Maslow and Rogers, for the trend beyond mere maintenance of psychic equilibrium is a striving towards self-actualization. It is this, and in general, only this that is touted as the end-all of effective personality development. Nonetheless, it is right here that greater clarification would have been useful. Coleman places ample emphasis on those characteristics which lead towards adjustive behavior. He even includes a section on creative thinking. Yet, it seems to me, he does not give enough emphasis to the features of creative living and to the healthy personality per se (a little over five pages out of 566).

There are some noteworthy and commendable 'old-fashioned' ideas in this book. For example, the principles and insights of the general field of psychology are used to explain the development of personality. The author emphasizes such topics as motivation, emotion, development, and learning as they are related to effective behavior. One reads. once again, of the effect of constitutional factors, in addition to social and emotional learning and experience, as determinants of the level of adjustment. One is also told of the kinds of conflicts that our American culture itself imposes upon the individual, and of the capacity of the individual to meet and resolve these conflicts as determined by internal as well as external factors. In any given phase of discussion, the reader is afforded a resume of almost all the major theoretical views that are pertinent. Hence, he can read of the latest work of Harry Harlow on the nature of love, Hans Selye's work on the psychophysical aspects of stress and adjustment, the findings in group dynamics, communication theory and cybernetics, or sometimes he can sample the potpourri of articles by some of the great thinkers in behavior theory today, in a sort of companion text of selected readings appendixed to the book itself.

THERE are some aspects of the book, however, to which I dissent. For instance, the forthright acknowledgment of values held and offered as worthwhile, is in itself good, but the basing of some of these values more on faith than on fact is to me questionable. The book states its purpose as being "to present the core findings of modern psychology." Man is described as a "unique creature [italics mine] with the capacity to assign value and meaning to his experience, to select creatively, and to choose and initiate a course of action" (p. 289). If nothing else, this comment ignores a considerable amount of research in comparative psychology. Another a priori assertion is that "science and reason can support but never supply the values that give ultimate meaning to human life and enable men to answer their persistent questions about such problems as suffering, evil, and death. For these final truths, mankind has traditionally looked to religion, whose answers derive primarily from revelation—the disclosure of God and God's will to man" (p. 306). Let me also say, in spite of my own respect for Maslow's concept of self-actualization, that there exists but insufficient evidence for this concept to justify

bearing down on it so heavily—except, of course, the consensual validation of men like Jung, Goldstein. Maslow, and Rogers, and the few empirical tests of Maslow's theories to which even I can acknowledge having been able to contribute. My final comment—hardly a complaint—is that this text is not written in a watered-down style that renders it more suitable for senior kindergarteners than for college students.

Every Word a Gem of Many Facets

Robert E. Pittenger. Charles F. Hockett, and John J. Danehy

The First Five Minutes: A Sample of Microscopic Interview Analysis. Ithaca, N. Y.: Paul Martineau, 1960. Pp. xii + 264. \$6.50.

Reviewed by Joseph D. MATARAZZO

Dr. Pittenger, an MD, is Director of Research and of Psychiatric Services at the George Junior Republic in Freezille, New York. He has held appointments at the Yale University School of Medicine and at the Upstate Medical Center of the State University of New York and has served as Director of the Onondaga County Mental Health Clinic in Syracuse. Dr. Hockett, a PhD, is Professor of Linguistics and Anthropology at Cornell University and author of the widely used text. A Course in Modern Linguistics (1958), and of articles on Mandarin Chinese and on the Potawatomi Indian language. Dr. Danehy, an MD, is Assistant Professor of Psychiatry at the Upstate Medical Center of the State University of New York in Syracuse and Chief of the Psychiatric Section of the Syracuse VA Hospital. The reviewer, Dr. Matarazzo. a PhD, is Professor of Medical Psychology at the University of Oregon Medical School in Portland. In CP during the last four years he has reviewed books on dynamic psychology, the sociology of medical education, psychology of nursing, and clinical training in psychology (see CP's excellent indices). For the past eight years he has been

pushing research on the noncontent dimensions of behavior in interview—the frequency and duration of speech units, elements that 'carry' the content of communication,

Barely a decade ago investigators who were studying the content of psychotherapeutic interviews appeared to be unable to proceed beyond such variables as Noun-Adjective ratios, the Distress-Relief Quotient, and similar seemingly primitive measures of content-analysis. Recently there have been several books which seem to have made important new contributions to the study of verbal communications between patient and psychotherapist (e.g., J. Dollard and F. Auld's Scoring Human Motives, Yale Univ. Press, 1959: H. L. Lennard and A. Bernsteib's The Anatomy of Psychotherapy, Columbia Univ. Press, 1960; H. H. Strupp's Psychotherapists in Action: Explorations of the Therapist's Contribution to the Treatment Process, Grune and Stratton. 1960).

The First Five Minutes appears to this reviewer to be another of these significant contributions to the study of communication in psychotherapy inter-

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To be published this fall, 288 pages



Appleton-Century-Crofts, Inc. 34 West 33rd Street, New York 1, New York

views. The research reported in this book was the collaborative effort of a linguistic anthropologist and two psychiatrists. These investigators spent what appears to be several years on a painstakingly detailed analysis of a fiveminute interaction between a psychotherapist and a patient. The five-minute episode which was put to such a microscopic analysis was the opening fiveminute exchange in the initial interview recorded both in print and on sound by M. Gill, R. Newman, and F. C. Redlich and entitled The Initial Interview in Psychiatric Practice (Internat. Univ. Press, 1954).

The unique method of content analysis employed by Pittenger. Hockett, and Danehy involved the simultaneous presentation (by two different sets of symbol systems) of the linguistic (phonemes) and paralinguistic (e.g., sighs, drawls, slurs, inhalations, loudness and softness, breathiness, speech coughs, etc.) behavior of each speaker. The book's heart, some 176 pages or 70 percent, is that portion of it, where, with the aid of their two systems of symbols. the authors note the pronunciation of successive words spoken by the psychotherapist and the patient, the intonations, the locations, and durations of pauses, the hems and haws, the sighs, gasps, coughs and throat-clearings, and such variables as rates of speech, register, volume, and tone quality, and then relate all these paralinguistic happenings to the words employed by the two persons in interaction during the five-minute period. Such linguistic and paralinguistic transcribing of spoken English is a skill apparently available to only a handful of specialists today, a fact which will make the use of this method of content-analysis by other investigators difficult if not impossible. Another factor which may inhibit other research workers is that, even in the hands of such a specialist, merely the transcription of a five-minute interview required between 25 and 30 hours!

After preparing the transcription the authors had numerous plenary conferences in which, with the aid of their 3-row analysis (i.e., 1: ordinary spelling; 2: linguistic analysis; and 3: paralinguistic analysis) of the transcript before them, they repeatedly listened to

a tape-recording of the five-minute episode—listening over and over to a single word, a whole sentence, or a whole passage. As they listened and read the transcript in this very careful way, they were able to add still another dimension to their analysis, namely, interpretative comments which occurred to one or another of them as to the *meaning* of the word, passage, or paralinguistic event.

As might be expected from the fact that two of the investigators were psychiatrists, many (but not all) of these proffered interpretive comments as to what was being communicated were cast in psychiatric and psychodynamic frameworks. Examples of some of the paralinguistic signals and their contextual meanings (given here in parentheses immediately after each signal) are as follows: 1: quavery voice (anxiety); 2: overloud speech (anger); 3: squeezed voice (depression); 4: word fracture (momentary childish embarrassment); 5: momentary opaque intonation in the therapist's speech (communicates a brusque Go on!, suggesting to the patient that there must be more she has to say on this topic).

An example of another form of their approach to interpretation is given in the following analysis of one of the patient's early comments; i.e., the question "May I smoke?" The authors interpret this question, in its context, as follows:

This is two questions at once. In words, it is the question it obviously seems to be. The other is carried by the style of delivery: the slight breathiness, slight oversoft, second-degree overhigh, and the clipping on smoke and its peculiar release. For some women this combination of features would be kittenish. For P, it seems rather a sort of pseudo-kittenishness, an out-of-character style adopted for the moment precisely to put the second question: "What sort of situation is this? Do I have to ask your permission to smoke? What is

our relative status? What things can I do on my own and what things must I get your permission for?"

The timing of the double question is significant. T has just reassured P (in T5b) that there is no rush-so that time out for a question about smoking and to light a cigarette will not matter. The apparent need for a cigarette is also important. P's tension at the beginning of the interview has now decreased to the point that she can think of a cigarette. Yet possibly the need is itself an indication of continuing tension, which the activity of smoking may help to alleviate. The uncertainty on this last point is that we have little evidence as to what sort of a smoker P is. The only direct evidence on the tape is the ensuing sound of lip-rounded sighs (e.g., at the beginning of P11), which probably means that she is expelling smoke through her mouth, and thus that she inhales.

It will be clear to the reader that in this analysis of one question the authors are freely intermingling hypotheses and inferences drawn from linguistics, psychiatry, and other sources.

Since their book describes a pilot project to test their method, no data for reliability or validity are given by the authors. However, although the approach is definitely clinical and intuitive, the very careful analysis performed of this five-minute episode in the lives of two people appears unusually insightful and very well may represent a significant contribution to psychotherapeutic research. The reviewer approached this book with extreme skepticism regarding such a detailed (and picayune, he thought) method of analysis. He finished the last page having gained respect for its potential. Although probably few students of psychotherapy will want to carry out such minutely detailed microscopic analyses of verbalizations in therapy, nevertheless, they may find in this book suggestions which may help them the better to understand interactions in psychotherapy.



What is a good experiment? It is that which informs us of something besides an isolated fact; it is that which enables us to foresee, that is, that which enables us to generalize.

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Edited by SIGMUND KOCH, Duke University. Volume IV available January, 1962.

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Personality's Frontiers Attacked

Riley W. Gardner, Philip S. Holzman, George S. Klein, Harriet B. Linton, and Donald P. Spence

Cognitive Control: A Study of Individual Consistencies in Cognitive Behavior. (Psychological Issues. Vol. I, No. 4, Monograph 4.) New York: International Universities Press, 1960. Pp. 185. \$4.00.

Reviewed by Charles M. Solley and Samuel Messick

Gardner and Holzman are Kansas PhDs who are now at the Menninger Foundation, the former as director of the cognitive research group, the latter as a senior psychologist who is also teaching at the Topeka Institute for Psychoanalysis. Klein and Spence are Columbia PhDs (Spence from Teachers College) and Mrs. Linton a Vale PhD, and they are all at New York University's Research Center for Mental Health, Solley, the first reviewer, is an Illinois PhD who was recently the co-director in a problem of perceptual learning at the Menninger Foundation and is now Assistant Professor of Psychology at Wayne State University. In 1960 he published Development of the Perceptual World with Gardner Murphy (Basic Books; CP will review it shortly). Messick, the other reviewer, is a Princeton PhD, who, after using a Ford Foundation Fellowship for research in the dynamics of personality at the University of Illinois, is now principal investigator in two projects on the organization and assessment of personality at the Educational Testing Service in Princeton.

In recent years considerable energy has been invested in exploring the ever-fresh frontiers of personality by following the signposts of perceptual and cognitive phenomena. Instead of working in a single direction or attempting to extend the knowledge of a single cognitive variable to the limits of its generality, as has been a custom in this field, the present authors describe a research project in which six cognitive variables, represented by some

thirty experimental measures, are interrelated in a broad context of personality organization. Unfortunately they used an extremely small sample.

The six primary variables in this study reflect stylistic consistencies in cognition and have been conceptualized by Gardner, Klein, and their co-workers as regulatory tendencies manifested in typical modes of perceiving, remembering, and thinking. Historically these cognitive consistencies have been referred to by various names such as perceptual attitudes or cognitive systemprinciples, but the present monograph favors the term cognitive control because of its attendant emphasis upon regulatory structures. These cognitive controls are characterized within the theory of psychoanalytic ego-psychology as conflict-free processes which serve adaptive functions in relating the self to the environment and possibly as modulating functions in mediating drives and needs.

The authors devote separate chapters to each of the six variables of cognitive control-leveling-sharpening, tolerance for unrealistic experiences, equivalence range, focusing or scanning, constricted-flexible control, and field dependence-independence-and an important feature of the discussion is their explicit treatment of rationales for the experimental measures as well as a description of the specific test procedures. This welcome attempt to provide an immediate theoretical basis and interpretation for experimental measures is followed throughout the monograph. supported by relevant perceptual theory, sometimes heavily slanted in the Gestalt direction and interwoven with the psychoanalytic theory of personality. In a real sense, however, the authors have tried to write two monographs, one theoretical and one experimental, and in straddling the fence they have regretably sacrificed some of the clarity, unity, and completeness of each, perhaps partly because of inevitable space restrictions.

Most of the experimental findings in the monograph relate to an appraisal of possible interrelationships among the six cognitive control variables, as represented by 33 measures administered to 30 males and 30 females. Because of possible sex differences in the patterning of correlations, factor analyses were performed separately for each sex. In addition to the obvious point that the small size of the sample renders the conclusions tenuous, particularly those dealing with the establishment of factor dimensions and their interrelationships, it must be noted that the nearequality in number of subjects and number of variables does not provide the required degrees of freedom for an appropriate application of the commonfactor model itself. This condition was partly alleviated, however, by the fortunate finding of similar factors in a subsequent analysis of 16 selected measures.

The authors infer the existence of two factors, that account for only 20 per cent of the total variance, for the males in terms of scanning and tolerance for unrealistic experiences, and three factors, that account for approximately 35 per cent of the total variance, for the females in terms of field articulation, leveling-sharpening, and equivalence range. This difference in factor structures may reflect differential preferences for cognitive controls between the two sexes, but it is more likely due to sampling instabilities. The results, then, are at best only suggestive, but the suggested dimensions may play such a potentially important role in the organization of cognition and personality that their possible regulacory influence must be considered seriously. Certainly further research should be encouraged.

Wives Tell about Marriage

Robert O. Blood, Jr., and Donald M. Wolfe

Husbands and Wives: The Dynamics of Married Living. Glencoe, Ill.: Free Press, 1960. Pp. xxii + 293, \$5.00.

Reviewed by George Levinger

Blood is Associate Professor of Sociology at the University of Michigan and the author of Anticipating Your Marriage (Free Press, 1955; CP, June 1956, 1, 183f.). Wolfe is Research Associate in the University of Michigan's Institute of Social Research. Blood is a sociologist interested in teaching, research, and counseling. Wolfe is a social psychologist concerned with research and theory on small groups. The reviewer, Levinger, is a social psychologist. Associate Professor in the School of Applied Social Sciences and in the Psychology Department at Western Reserve University. He too is concerned with the study of small groups, and just now is finding out about cohesiveness and consensus in that quite small group, the husband-and-wife.

The attempt to place the dynamics of married living under scientific scrutiny may help to shatter illusions about one of the few remaining areas of man's ignorance of his world—his relations to his spouse. Man's illusions have been damaged in turn by Copernician astronomy, Galilean physics, and Darwinian biology; most recently, Freudian and other schools of psychology have impaired the image of college student and college rodent alike. Now, in trying to illuminate the sanctuary of family life, social science encounters new problems and new vistas.

Armed with results of a careful probability survey of 731 Detroit housewives, supplemented by 178 interviews with rural wives, the authors of this book attempt to give us some systematic glimpses into a cross-section of husband-wife relationships. Since both authors are at the University of Michi-

gan—where Blood teaches sociology and Wolfe does research in social psychology—they benefited in their study from the facilities of the annual Detroit Area Study. These facilities enabled them to obtain a representative sample of respondents from the metropolitan community, untainted by any association with a marriage and family course at the University.

By the "dynamics of married living" the authors mean certain aspects of family structure and function. Family structure, handled in two early chapters, refers to relative decision-making power of the spouses and to their division of labor. Family functions are discussed in five chapters on economic matters, child-bearing and rearing, companionship, communication on emotional matters, and satisfaction with marital love. A final chapter addresses itself to "stresses and strengths in American families"-the extent to which husbands and wives have disagreements and their degree of satisfaction with their marriage. Each topic is seen as some form of a dependent variable, and each is discussed in separate chapters. Each variable is, in turn, related to about twenty or so independent variables, that represent demographic, social, and economic factors, the family life cycle, and some other topics.

The book's contribution lies in its descriptive job. Each individual chapter presents a fascinating capsule of information about married life, as the wife reports it. It is interesting to note, for example, how a husband's relative power to make decisions (principally in economic matters) is associated with his income, education, religion, frequency of church attendance, and the stage of

his family's life cycle. For example, the husband's power is found to decline with decreasing income and with increasing length of marriage. From the data provided, it is concluded that the greater the relative resources of either marital partner, the greater will be that partner's power to make decisions in the family.

THE association between distribution of resources and marital power is only one of many topics considered in this book. One difficulty with the presentation is the plethora of information and tabular data to which the reader is exposed. Altogether there are 133 tables planted throughout the text. Yet rarely do the authors relate the findings of one table explicitly to those of another, especially when the tables are in different chapters of the book. It is, therefore, difficult to know just what are the book's most crucial findings. Every separate chapter is well organized and interestingly written. Most chapters show conceptual sophistication, although occasionally lay moral judgments creep into the text. Yet while each chapter constitutes a relatively satisfactory unit, the total book fails to achieve unity. Having finished it, one is at a loss to decide which of the findings are the most important, or what propositions are supported or infirmed by the study. The authors' concluding observations, that "marriage is here to stay" or that marital "strength ordinarily comes from children" are hardly derivable from their data.

It was disappointing to discover no integrating theme for the many findings, although the authors seemed on the verge of introducing one in their early chapter on resource-balance and marital decision-making. Such a concept as resource or control of means would have been appropriate for subsuming their subsequent findings. Thus, the division of labor in the household, the economic goals of marriage, childbearing preferences, companionship, and marital satisfaction, could all have been systematically related to available marital resources-either real, ideal, or expected. Essentially, the pleasures and problems of married living depend on the deployment and the sufficiency of the available resources, whether material, social, or emotional.

A further obstacle to drawing conclusions from this work resides in the limitations of the survey method. One of these limitations is the reliance on one marital partner for painting a picture intended to be valid for both. A second is the use of direct self-report for measuring such variables as marital satisfaction.

Even if one assumes that the housewives gave valid responses, it is still hard to draw clear-cut inferences from these survey data. For example, one table shows that the average Negro wife has greater power in her family than the average white wife, but the analysis does not show to what extent this difference is a function of race itself, or of income, occupation, and other variables. Apparently it was not feasible to subject the data to crossbreaks or partial correlations.

Another difficulty inheres in the frequent statements of cause and effect: e.g., "children are a source of strength in marriage, provided there are not too many of them" (p. 262). This dictum is based on a finding that three-child families scored highest on marital satisfaction (though they also scored high on the key variable of economic status). No evidence is cited, however, to lead one to believe that the introduction of three children will place one on a plateau of marital bliss from which he will be dislodged by new additions. Such evidence is unobtainable. In drawing their conclusions, the authors ought to have confined themselves more closely to their data.

This book is beset by theoretical and methodological questions which plague the field of family research. Yet, despite these criticisms, it marks a definite advance in systematic and representative analysis of the relations between husbands and wives.

Singly upon the equal heights
Enthroned at last where she belongs,
She takes no pleasure in her Rights
Who so enjoyed her Wrongs.

-PHYLLIS McGINLEY

Neither the Soul

S. L. Rubinstein

Grundlagen der allgemeinen Psychologie. (2nd rev. ed.; trans. from Russian into German by H. Hartmann.) Berlin, East Germany: Volkseigener Verlag, 1959. Pp. 876. DM. 17.—,

Reviewed by FRANK WESLEY

The author, Rubinstein, is a member of the Institute of Philosophy of the Soviet Academy of Science in Moscow. He has published texts on the foundations of general psychology in 1935, 1940, 1946, and 1958. This book has been approved, disapproved, and now reapproved by the Soviets (cf. Razran, CP. Apr. 1957, 2, 96). The reviewer, Wesley, is Instructor in Psychology in Portland State College in Oregon. He was born in Germany, educated abroad, trained in psychology at Washington State College (PhD, 1958), and, with a cosmopolitan background, tries, when not working at animal comparative psychology, to coordinate Russian, German, and American psychology-and sometimes French too. He has taught a course on foreign trends in psychology. See his review of Teplov's Psychologie, Volks und Wissen (CP, Sept. 1960, 5, 3061.).

A POTENTIAL threat in psychology, this book has been once in and once out of political favor but is now introduced again as the standard Russian historical survey by its East German publisher. Few books written by a single author contain so much information about normative philosophy, psychological history, and detailed general psychology, but the intermingled presentation of these three areas detracts greatly from its strength,

The mixture of philosophical valuejudgment and history begins with a negative criticism of all systems related to the mental aspects of Cartesian dualism, such as parallelism, interactionism, panpsychism, structuralism, psychophysics, and others. Pleonastically they are described as "rationaler Idealismus" or as "induktive Metaphysik." Expecting from this beginning a favorable acceptance of materialistic systems, the reader is surprised to find the same severe criticism of all systems disregarding or de-emphasizing the mind. "Vulgärer Materialismus" is ascribed to the efforts of Leroy, Cabanis, Holbach, LaMettrie, Locke, Büchner, Haeckel, and J. B. Watson. According to the author the proponents of these extreme systems have misled psychology. Descartes did so by introducing the soul to psychology and Watson by taking the psyche out of it.

The adherence to either pole of dualism is considered to have created a 'methodological crisis' which found its basic solution not in the work of Fechner and Wundt, but in the "Synthese" advanced in the writings of Marx and Engels. This synthesis is considered absolutely necessary for any logical and scientific system, and ergo for "unsere" (our) psychology. It is advanced throughout the entire book by stress upon the unity between "Objektiven und Subjektiven . . . Realen und Ideellen . . . Erleben und Wissen." At no point is this permeating synthesis described in even the faintest operational terms, thus making it possible for the author to criticize systems which attempt unification. Herbart, e.g., is too Newtonian, Fechner too eclectic, Galton too absorbed in "bürgerlicher Testologie," whereas Stern reduces personality to "abstrakten, metaphysischen Kategorien." In contrast to the history Boring presents, one may get the impression from Rubinstein's treatment that most of Western psychology has been a product not of the marriage, but of the divorce of philosophy and physiology.

While the approach to Western psychology is biased, the history of Russian and Soviet psychology is treated rather objectively. From the eighteenth to the twentieth century the works of about fifteen different philosophers are discussed. At this point the emphasis turns to medical, pedagogical, and physiological fields, citing in detail the work and affiliations of many Russian

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scholars (Wagner, Korsokov, Tarchanov, Bechterev, Ushinski, Kornilov, Wygotski, Ladygina-Kohts, Borowski, Wojtonis, Lurjia, Smirnow, Setchenov, Leontjev, Pavlov, and many others). The only excuse for materialism is made in Bechterev's case; his approach is said to have been of value to counteract the idealists.

The historical part contains additional information which would be considered too specialized for most U. S. texts. The section on the historical development of human consciousness, e.g., exhibits detailed maps of Brodmann's architectonic areas and discusses other maps of Vogt, Economo, and Koskinas. Conversely, the sections on general psychology are permeated with historical facts. The histories of developmental psychology and linguistics which could well represent a separate book are added in parts to the general sections of perception, cognition, and conation.

This rich mixture of subjects and topics makes the entire book interesting and advantageous to the reader who has previously been exposed to presentations such as Heidbreder and Woodworth. The advanced student who can separate the normative judgments and integrate the enormous amount of data imported from the related fields of anthropology, art, literature, economics, and politics would benefit most from this book. Undergraduate students would have difficulty in relating the closely interwoven presentation in spite of the 235 subdivisions listed in the table of contents. The handbook-like amount of historical and general data would be more valuable had the author explained his rationale for subdividing certain topics. Thinking, e.g., is discussed under the headings of character of thinking, theories of thinking, basic phases of thinking, basic operation of thinking, and basic forms of thinking. Clarity is sacrificed because many key terms are not defined. Determinism is criticized, yet Tolmanian 'purpose' is accepted if applied to "Tätigkeit" (activity) but not if applied to "Verhalten" (behavior), and it is left to the reader to infer that 'activity' seems to satisfy the Marxian synthesis by symbolizing socially purposeful behavior. Some direct

quotations from Marx make Marx appear, however, more like an "industrial behaviorist' than a 'purposivist.'

In spite of its tremendous amount of general information, the book is seen to be mainly of historical interest because its 440 references, ranging from 1856 to 1958, show a median date of 1928, with only 29 references after 1940. Razran (CP, Apr. 1957, 2, 93-101), in a comparison of Russian texts, found Rubinstein's 1940 edition atypical in quoting only one Paylovian reference and singular in presenting non-Russian references other than Marx and Engels. These characteristics are essentially maintained in 1959. Though Pavlovian references have increased to 22, about threefourths of all the subject matter and three-fourths of all the references are of Western origin. For example, from eight books cited under general historical surveys, seven are Western (Boring,

1929; Flügel, 1933; Klemm, 1911; Murphy, 1929; Pillsbury, 1929; Ribot, 1875; Spearman, 1937) and one Russian (Troycki, 1867).

It seems incongruous that the author should criticize almost all Western systems and methodologies and yet lean so heavily on them. One wonders how much the Western work actually contributed to the synthesis he advocates so strongly. The reader cannot help agreeing with him that this synthesis of subjectivism and objectivism is and has been one of the most pertinent problems in psychology; yet, although one is stimulated to critical thought, he will not find in the book particular directives as to how to solve this persistent problem. Nevertheless, in spite of this difficulty and others, the author must be respected for the enormous amount of psychological information that he offers in his volume.

Conquering Stress with Fear

Irving L. Janis

Psychological Stress: Psychoanalytic and Behavioral Studies of Surgical Patients. New York: John Wiley & Sons, 1958. Pp. xiv + 439, \$6.95.

James L. Titchener and Maurice Levine

Surgery as a Human Experience. New York: John Wiley & Sons, 1960. Pp. xxx + 285.

Reviewed by Walter L. WILKINS

Janis is a Columbia PhD who was trained psychoanalytically at the New York Psychoanalytic Institute and who is now Associate Professor of Psychology at Yale. He contributed to The American Soldier (Stouffer et al., Princeton Univ. Press, 1949) and to Communication Persuasion (Yale Univ. Press, 1953) and is author of Air War and Emotional Stress (McGraw-Hill, 1951). Titchener is Assistant Professor of Psychiatry at the University of Cincinnati. He is a Duke MD with Army experience. Levine is a Hopkins MD, Professor and Director of the Department of Psychiatry at Cincinnati and author of Psychotherapy in Medical Practice

(Macmillan, 1942). The reviewer, Wilkins, is Scientific Director of the U.S. Medical Neuropsychiatric Research Unit in San Diego. During World War II he was a psychologist attached to the U.S. Navy and after that Professor of Psychology in St. Louis University. He reviewed Ginzberg's The Ineffective Soldier (Columbia Univ. Press, 1959; CP, Aug. 1960, 5, 243-246).

Surgical intervention for the purpose of halting, palliating, modifying, or preventing the course of human illness is the beginning of a complicated process.

This generalization by James Titchener is most adequately docu-

mented in these two books. Almost every adult has experienced some surgery or at least been close to someone who has had such experience, so the emotional concomitants of surgical experience are familiar enough to make everyone a sort of amateur expert on how it feels to undergo the knife; but, as in so many things in which the experience of the amateur is universal, the folklore almost obliterates known fact.

There is a good deal of practical information in surgical texts and in what is handed down through hospital scrubbing-up sessions or postoperational discussions on how patients react. This information, while practical, is also largely anecdotal and must suffice until controlled scientific study of the surgical patient and his reactions is more advanced. Both Janis and Titchener have, in these two volumes, done much to systematize what is known about patients' attitudes toward surgery but from somewhat different but complementary vantage points.

ANIS' book includes a single case, a group of hospital interviews, and a questionnaire study of college men. The single case is a female patient in psychoanalysis for two years whose operation provided her analyst. Janis, with copious materials for studying anticipation and retrospection about her surgery. Hypotheses developed in the analysis of the patient are tested against data from the interviews of the thirty routine surgical cases seen in hospital. The degree of fear manifested before major surgery and the stress tolerance shown after operation were compared with the intensive material on the woman. The questionnaire sample of 149 men who had had recent surgery and 100 others who had dental drilling (found through questionnaire to a thousand collegians) provide further testing of the hypotheses.

From his analyzed case, Janis has developed fourteen major hypotheses and a good number of supplementary propositions. Central to most of the hypotheses is the reality that objective threat of body damage induces fear in normal people. Anxiety before an operation can be highly useful in preparing the patient emotionally for the surgical ex-

perience. The type of emotional relief experienced after operation is directly related to the kind and amount of anticipatory fear experienced. Janis shows. and Titchener's data support, that a moderate degree of such fear is desirable, but an extremely high degree is likely to result in intense operative fear and postoperative difficulty in adjustment. Further, an extremely low degree of anticipatory fear is likely to result in postoperative resentment toward authorities involved. Janis' discussions. from analytic theory and his analyzed case, are cogent and ingenious in tracing the features of early family experience of possible relevance.

THE volume by Titchener and Levine was inspired by an address of Levine's, which is now reprinted as an introduction, but is otherwise really a book by Titchener, who writes the preface in the first person singular and acknowledges his indebtedness to Levine in it.

This report consists of a study of two hundred surgical patients, randomly selected in a municipal hospital with mostly a lower socioeconomic clientele. Daily, during the hospital stay of a patient, a member of the investigating staff conducted an associative anamnesis type of interview. Cross-checking was managed by obtaining from the patient the name of someone close to him, who was then interviewed by a social caseworker. Surgical conditions affecting the patient were checked by special notes from the surgical resident, and literacy and other conditions were determined by administering the Wechsler-Bellevue test to 47 per cent, the Rorschach to 55 per cent, and the MMPI to 25 per cent of the sample. These dossiers were studied by three investigators to determine the presence of diagnosable psychiatric disorder.

Titchener is quite aware that this method has some dangers, such as the confusion of environmental limitations (53% Negroes) with personality limitations, the confusion of personality limitations with psychopathology, or just the plain missing a diagnosis. Less than one patient in ten received a clean bill of mental health and most of them had some character or behavior disorder. This incidence is so surprising that

Titchener wonders whether illness and admission to the hospital might in itself be stressful enough to initiate or to aggravate adaptational difficulties, whether surgical illness could possibly be an eventuality growing out of psychiatric disorder, or whether the socioeconomic level of the sample tapped patients of higher incidence.

What a threat to human personality surgery must be is also suggested by Titchener's follow-up information on 128 of his sample. Seven out of ten were improved surgically, but only one in four had any improvement in general adjustment. The other three of four persisted in poor adjustment despite surgery, or actually became worse.

Of Titchener's 200 patients, 45 were beyond age 65. Of these older patients, 11, just about one-fourth, suffered permanent organic cerebral damage (in noncerebral operations), and these were most likely in-patients who lived isolated or lonely lives. Of the 200, 44 had psychotic reactions to surgery (or perhaps were psychotic on admission). Of these, 22 had a delirium of acute brain sydrome, and seven of these deliriums did not remit. Furthermore, 90 per cent of Titchener's aged surgery patients had depressive feelings following operation.

His is not merely a matter of the cheerless word or the apathetic patient who never has a visitor, for it is also the life pattern and the closeness of the familial or parafamilial ties. We depend on the group we are closest tofamily or squad or team. The soldier in battle depends on his group, all of whom are capable of supporting each other in danger. Upon whom does the lonely patient, facing operation, depend? Fear before battle is normal; fear before operation is normal, too. In fact, as Janis emphasizes, "relative absence of anticipatory fear is pathogenic in that the individual fails to build up effective psychological defenses in advance and, therefore, finds it difficult to ward off feelings of helplessness when danger actually materializes." and "the high degree of emphasis on being loved by members of one's family may function as an important means of bolstering self-esteem and of preventing reactions of post-operative guilt and aggression."

SELF, SOCIETY, EXISTENCE

By PAUL E. PFUETZE

Human Nature and Dialogue in the Thought of George Herbert Mead and Martin Buber

"Will puzzle and delight everyone having more than a casual
interest in the sciences of man—
philosophers, psychologists, psychiatrists, sociologists, anthropologists, theologians, humanists,
of every description. Paul Pfuetze
ingeniously proves that George
Herbert Mead and Martin Buber,
two of the influential thinkers of
the 20th Century, hitherto supposed to be an infinity removed
from one another, exhibit critical convergences at the core of
their systems."

-BENJAMIN NELSON

"After stating the principal concepts and beliefs of each writer, the author proceeds to a careful analysis and comparison of their views, indicating the prominent similarities and differences of their metaphysics and social philosophies."—Psychiatric Quarterly Supplement

"Another and significant item in the constantly increasing number of books . . . that deal with the self as one of the central and crucial concepts of a human psychology. Pfuetze's position on the centrality of the self is identical with that of symbolic interactionism: human psychology is social psychology."—The American Journal of Sociology

■ Dr. Pfuetze is Professor of Philosophy at Vassar College. With this book the author pays tribute to his College on the occasion of its Centennial

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And Titchener points out that "the most potentially severe psychopathological reaction just this side of an overt psychotic break is the phenomenon of denial of illness."

Denial, as an intellectual process, to Janis, serves the patient's purpose by warding off unpleasant affect by failure in appraisal of some past, present, or future predicament. Pathological denial includes avoiding clear-cut evidence of danger or loss; nonpathological denial includes avoiding ambiguous evidence of possible danger.

Refusal of surgery when health or even life depend on it is certainly a psychological reaction and is related to delay in seeking diagnosis and treatment. Titchener points out that this delay is not a matter of age, sex, finances, intelligence, education, or degree of psychiatric illness. What then is it a matter of? Complex as the answer to this question must be, it involves the fact that delayers fear illness too much, perhaps because of fantasies of harsh treatment; they fear something else moresomething that may involve loss of secondary gains and avoidance of responsibility; and they look upon illness as a sign of unacceptable personal weakness.

The question of who seeks surgery and who avoids it or of who seeks or avoids any medical treatment may be too complex to answer at present, but from these books one can obtain some challenging speculations and some testable hypotheses. Cosmetic surgery has intrigued psychiatrists for some time, and it may be a general rule that any plastic surgery not demanded by rehabilitation calls for a rather full psychiatric work-up, since there is considerable probability that motivation for magical results is overdetermined. Yet some refuse plastic surgery when rehabilitation calls for it.

Another interesting question raised is: who is a good patient? The answer seems to depend on who is asked the

question. The ward-attendant may say that it is the patient who is docile, uncomplaining, and nonresistant. The physician may say that it is the patient who shows cooperation by getting well.

JESSONS for the surgeon and for the psychologist associated with a surgical service are plain and relevant in these two volumes. Janis says "Advance information about post-operative pains and discomforts raises the level of anticipatory fear and subsequently makes for a decrease in the incidence of postoperative resentment." In pediatric hospitals, the staffs have gained some skill in telling children what is impendingperhaps for adults we should exercise similar care. Janis' notes on a physician and his reactions, and the evidence in Max Pinner's and B. F. Miller's When Doctors Are Patients, show that information of even a medically informed sort may not be sufficient to enable the patient to mobilize his resources to meet the anticipated danger and so to return to normal postoperatively. The patient not only needs to know but also to face up to the implications of the

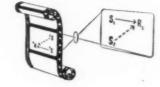
Methodological contributions certainly include Janis' discussion of the differential validity of the intensive contemporaneous case study as contrasted with the questionnaire asking retrospectively about attitudes. Greater validity of the single case is the result of the greater reliability of contemporaneous report as contrasted with retrospective report, of the immediate effects of personal confrontation as contrasted with a pencil-and-paper or classroom contact, and of the possibility of immediate checking as contrasted with the social desirability and other distortions of duplicated instruments. The closely reasoned defense of the validity of psychoanalysis will probably be much more acceptable to the average psychologist than have been some previous attempts.



We see that which we are, and our eyes project on every side an image of ourselves . . .; we scatter our sins broadcast and call them neighbors; let us scatter our virtues abroad and build us a city to live in.

INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



Adventure or Fundamentals

TMI-Grolier

Fundamentals of Algebra. Albuquerque, N. M.: Teaching Machines, Inc., 1961. Pp. 242. \$10.00.

Norman A. Crowder and Grace C. Martin

Adventures in Algebra. New York: Doubleday, 1960. Pp. 350. \$3.95.

Reviewed by HARRY F. SILBERMAN

As in the case of the TMI programed textbook Inferential Statistics, previously reviewed by Saltzman (CP, March 1961, 3, 106f.), no authors are identified for Fundamentals of Algebra: apparently this was a corporate effort, and Saltzman's plea for authorship identification has thus far gone unheeded by Messrs. Homme, et al. of TMI. Norman A. Crowder is Manager of Training Systems Department, Western Design Division of U.S. Indstries, Inc., Santa Barbara, California, He attended Reed College and did graduate work in psychology at the University of Chicago, where he also taught statistics. Another of his "TutorTexts" is The Arithmetic of Computers (Doubleday, 1960; CP. Jan. 1961, 6, 25f.). His co-author, Grace C. Martin, is a writer on the TutorText staff. She graduated from Kalamazoo College, and was a technical writer for the armed forces before joining Western Design. The reviewer, Dr. Silberman, received his EdD degree in Educational Psychology from the University of California at Los Angeles, and is now Associate Director of the Automated Teaching Research Project sponsored by the System Development Corporation. He has taught high-school mathematics, served on the research staff for the New York City Board of Higher Education, and taught courses in educational research at U.C.L.A. Dr. Silberman is currently a member of the American Educational Research Association's committee for developing evaluative criteria for autoinstructional programs and devices.

MONG those who are writing self-A instructional programs, the most popular subject seems to be mathematics. This review contrasts two methods of using such programs to teach elementary algebra. Self-instructional programs represent a curious mixture of textbook and test. Unlike a textbook. the content is usually not comprehensive; unlike a test, the program often is not accompanied by data concerning its effectiveness. In the absence of published data on what students learn and remember from using these programs, this review is limited to program content and programing methods. Since there are no absolute standards for evaluating either content or programing techniques, these features are contrasted for the two programs being reviewed. Admittedly, these programs are

trying to do different things and as such are not strictly comparable. Nevertheless, the potential consumer may be interested in comparative reactions to the two programs. Accordingly, they are compared on dimensions of general approach, feedback, adaptiveness, practice, style, content, and format. Reactions to the programs were solicited from several mathematicians, and each program was tried out with two high-school students.

The TutorText Adventures in Algebra is a 'scrambled book' in which the student reads a paragraph of material to be learned and then receives a multiplechoice test question. His choice of answer determines which item will be presented next. If the student passes the question, he is given the next item of information and the next question. If he fails the question, he is sent to an item where the information is reviewed and the nature of his error is explained to him. He is then returned to the previous item to try another option. A separate item of correctional material is provided for each wrong alternative. With each test question, incorrect answers take the student to items containing information designed to correct the error before he continues through the sequence. The scrambled-book method originated in trouble-shooting training procedures rather than in any existing learning theory. Conditioning models are considered inappropriate for teaching complex meaningful principles and ideas to human beings. The tutoring problem is viewed as a communication process, and the student's response is primarily a means of determining whether the communication process has been effective. If it has not been effective (that is, in the case of an error). the communication process is revised or repeated. This method places no constraints on the semantic content of the material

The TMI—Grolier programed textbook Fundamentals of Algebra is designed to condition the student to make crucial written responses to appropriate stimuli. The book is composed of a gradual, logical progression of instructional items in linear sequence. The student reads an item and writes one or more responses in the book opposite the item. Then he moves a cardboard mask to expose the correct answer, thus providing immediate knowledge of results. The semantic content of the items is designed to minimize the probability of incorrect responses. The TMI programed-textbook method has its basis in the premise that people learn what they do. Associations are formed between the stimulus items and the responses the student makes in the presence of those items. Provision for immediate confirmation of correct answers increases the probability that the items will be conditioned to the correct responses. The programing strategy is to elicit the desired response in the presence of the appropriate stimulus item by using some form of cue or prompt; when the prompt is withdrawn, the student can emit the desired response to the item without being prompted. A wide variety of positive and negative instances illustrating each principle is provided for concept formation.

The educational objectives of these programs might be inferred by what the student is required to do. The objectives of the TMI book are easily determined by scanning the answer column; the constructed responses constitute the goal behavior for the program. Objectives of the TutorText, by contrast, are evident in the subject-matter content; it is assumed that the form of the student response is irrelevant to learning.

In general, the TutorText relates computational techniques to basic ideas of algebraic structure. Definitions and properties are well formulated and there is some work with simple proofs. The emphasis is on the axiomatic structure of algebra and the role of the axioms in the proof of certain algebraic theorems. Not enough attention is given to the techniques for solving algebraic equations and transforming various algebraic expressions. This text would be most useful as supplementary material for the purpose of stimulating in the student an interest in mathematics or as a light review of important algebraic ideas for the student who has forgotten what algebra was all about. It is written as an introduction to mathematics rather than as a collection of tricks for solving problems.

On the other hand, the TMI text stresses algebraic manipulations and techniques. Some of the laws for addition and multiplication are stated but are not used deliberately to develop computational techniques. Rather, the student is told how to manipulate symbols so as to arrive at approved answers. This text would be useful for the student who begins his elementary statistics with an inadequate preparation in algebraic manipulation. The TMI book tells the student how to abide by the many conventions which plague the beginner, such as what to do first, where to put the dot, where to place the fraction line, and how to line up the equal signs. Rule-of-thumb procedures dominate the treatment. The students who were given this program learned the mnemonic aid for memorizing the five steps in solving an equation but said the treatment was "dull." If the student has the persistence to complete the text, he will have the recipe for obtaining two roots for a simple quadratic equation but will not understand why it has two roots. He will acquire techniques of transposing, cancelling, and putting terms in conventional order but he will know little about mathematics.

K NOWLEDGE of results is provided immediately following the response in both the TutorText and TMI book, but the similarity in feedback ends there. The TMI feedback messages are restricted to a few symbols or words and are not evaluative. The TutorText, by contrast, may give several sentences to explain why the student did well or poorly on an item. The correction is accomplished in a very supportive fashion. e.g.. "Your answer is technically incorrect. The trouble is that you are ahead of the argument." "You are to be congratulated on your knowledge of algebra, but at the same time. . . ."

While differences in rate of responding are possible with the TMI book, no provision is made for varying the sequence of items for different students. Each student traverses all items in fixed sequence; if he does not understand an item, his only recourse is to go back and review earlier items. The material is probably beyond the slower student's capacity and may bore the fast stu-

dent. The slower student becomes discouraged and the faster student becomes careless in reading, thus increasing his error rate. The students who tried both of these books were more restless and inattentive when reading the TMI book, and did not persist so well. They said that progress in the book was too slow.

In the TutorText, the material is adapted to the student insofar as the sequence of items depends on his performance. A single test question is used to determine whether a student comprehended the information contained in the paragraph preceding the question. If he fails to read the paragraph carefully, he should fail the test question and receive a correction item. Occasionally, however, he receives the corrective item for reasons other than faulty reading: the paragraph material itself is sometimes unclear, the test question is not answered in the paragraph material, the student is guessing (most questions have only two or three alternatives), or curiosity leads him to branch himself to the correctional item. Unfortunately for the student who is having trouble, the correction items are not always sufficient to clarify his difficulty once he is correctly branched to that material. The student who is doing well will skip over these remedial items. This skipping over pages hastens the completion of chapters and provides the reader with a sense of accomplishment which is added incentive to remain at the task,

While the TMI book and the Tutor-Text are self-contained and require no supplementary explanation, there is a distinct difference in the amount of practice required of the student. The TMI book uses short concise items, covers topics in very compact fashion and requires frequent student response. Review items are seeded throughout the program and substantial numbers of items are devoted to examples. Topical elements are presented separately and then combined and permuted in many ways. Topics were covered thoroughly and the students who completed the program had acquired some manipulative skills.

The TutorText items contain almost a page of reading material and take large steps between items (compared to the steps between items in the TMI book). Still, the uncertainty about what comes next in the TutorText may enhance interest for some students. While each item contains some review of the immediately preceding item, delayed review is very sparse (inequalities used in Chapter VI had not been used since Chapter I) and few examples are given. New terms are sometimes interjected in the text without prior introduction. While no prior mathematics is required to follow the text, a previous course in elementary algebra would be helpful in spots. Although the TutorText tells about algebra as a branch of mathematics, the lack of provision for practice or repetition makes it probable that manipulative skills will neither be acquired nor retained by the student with no background in the subject.

HE TMI book is monotonous and has a nose-to-the-grindstone orientation which results in the need for greater concentration or attention than the TutorText. This factor apparently produced more rapid fatigue and higher error frequency. The TutorText tells a much more interesting story about a greater range of topics, and (as already noted) the students showed greater persistence with it. The style is humorous and entertaining, but language is pitched at a higher level than the TMI book. It is not clear to this reviewer whether the preference among the sampled students for the TutorText is a function of the programing method per se or the relative writing skills of the authors.

The TMI book includes the fundamental operations and manipulations such as factoring, polynomials, fractional expressions, solution of equations through quadratic equations in one variable, and a treatment of inverse operations. The TutorText is broader in scope than the TMI book. Proofs are used to show that there is no largest prime and to develop the concept of a limit by summing a fractional series. Mathematical induction, irrational numbers, and an excellent treatment of negative numbers are also included.

Practical features of the TutorText such as chapter headings, index, and table of contents are omitted from the TMI text. The TMI text also gives the student one-fourth of each page to write his answers and another fourth of each page for a listing of correct answers. If the student uses the blank space to write his answers, the book will be consumed with one use. Where the method of use consists of two or three library copies purchased for reserved-book reference, writing in the text is clearly not feasible. In most items, the probability of giving a correct response is high as a result of the cues which are provided. The student knows he is correct before looking at the correct-answer column, and usually neglects to read the correct answers. A study by the staff of TMI indicated that the relevance of variables such as response mode and immediacy of feedback is inversely related to the probability of correct responding (J. E. Evans, R. Glaser, L. E. Homme, December 1960). Consequently, while the present format of the TMI book might be useful in a research tool for program improvement, the size of the book could be cut in half for the open market by omitting the space for writing in the book and the space devoted to repeating answers which are already given in the stimulus items. The space thus released could be used to avoid the occasional crowding of examples and problems within the frames.

Another format problem which puzzles the reader is why it is necessary to turn pages from back to front in the second half of the book. Unless data are available on the detrimental effect of occasional loss of stimulus control when the student notices a correct answer on the opposite page, it would appear more consistent with existing reading habits of most students to arrange all items to be read in the conventional manner—from front to back.

Some practical suggestions concerning the branching structure of the Tutor-Text occurred to the reviewer. For instance, branching decisions might be made more valid by using short sequential tests instead of single-item tests. Selection of each test item would depend upon previous responses to other test items. The decision on whether or not to give the student correctional material could depend upon the path he traverses through a branching tree of

test items. A given student need see no more than a few of the test items. The resulting measure may provide validity and reliability equivalent to measures obtained with a longer series of test items wherein each student traverses all test items in order. For research purposes it would be desirable to have a record of student responses as a basis for program improvement. The branching structure of the TutorText. however, is such that students who fail test items come back to the same track as students who made no errors. Provision for separating these students by means of the branching structure in order to keep track of their sequence could be made, although it would greatly amplify the size of the text.

Another problem noted was that single correction items were not always sufficient to clarify the student's difficulty. A subsidiary sequence of remedial items would probably be more useful for adapting instruction to individual differences. Perhaps the TMI fundamentals could be combined with Tutor-Text adventures by using the TMI items as subsidiary remedial sequences for the TutorText.

Unfortunately, no direction is offered by the authors of either text on how to use these books in the classroom. Nor were any data on their effectiveness supplied. Until empirical criteria are established, reviews of self-instructional materials will doubtless continue to be subjective. In spite of, or because of such lack of data, it is recommended that psychologists who are interested in conducting research on self-instructional methods should read these instructive examples of two contrasting approaches to teaching mathematics.

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But was there ever, since Christian Wolff's time, such a model of a German Professor [as Wundt]? He has utilized to the utmost fibre every gift that Heaven endowed him with at his birth, and he made of it all that mortal pertinacity could make. He is the finished example of how much mere education can do for a man.

—WILLIAM JAMES

ON THE OTHER HAND



ON RESOLVING CONTRARIETY: AUTHOR AND REVIEWER

In this letter, our first purpose is to discuss what we see as some misstatements and misinterpretations in A. R. Jensen's review of our book, *Toward Understanding Human Personalities* (*CP*, Nov. 1960, 5, 353–355); but, more importantly, we want to use this discussion to help explain the reasons for urging several additions to *CP*'s procedures.

The letters to On the Other Hand never express doubt about the importance of CP's reviews. They take for granted that CP has accomplished the 'impossible'—namely, making a "journal of reviews" a major journal in psychology. These letters, however, frequently voice protests, sometimes well documented, both about factual details and about over-all interpretations in the reviews. Many imply or say that these are especially unfortunate in CP because, since it is APA's official review journal, CP's reviewers are unavoidably given extraordinary influence.

In the past, the present writers noted these criticisms with occasional sympathy. Now, however, when involved personally, their mild academic interest has miraculously been transformed into effective reaction potential, and they find themselves writing to propose a plan (somewhat elaborating the ideas of W. S. Taylor described in CP Speaks in March 1961, 6, 73) which they believe would be both valuable and feasible for CP.

CP's editor has emphasized the fact that, since reviewers are humanly fallible, he is willing and even eager to provide space for thoughtful come-backs. In fact, from his presidential address on The Psychology of Controversy before the APA in 1928, psychologists might easily have predicted that Boring would value such give-and-take, despite its augmentation of editorial burdens.

The existing arrangements for discussion of reviews have served some very real functions, but they have these disadvantages. (1) Letters often deal with simple factual points, making corrections which the reviewers would have preferred to make if the errors had been pointed out to them beforehand. (2) On many factual details, any adequate documentation of the

corrections would take far more space than CP can afford to give, even when the points involved are so unequivocal that the reviewers would gladly have made changes could they have had before publication careful analyses on these points. (3) Many authors are reluctant to protest publicly regarding reviews, and mistakes and misinterpretations remain unchallenged in consequence. (4) Frequently the points at issue between author and reviewer reflect irreconcilable differences of outlook. In many of such cases, it would be exceedingly valuable for CP's readers to have an opportunity to compare the review at the time with some brief alternative statement by the author. It is inefficient to see such alternative statements only months later.

These difficulties, it seems, could be eliminated by the following procedure. (1) Each review could be sent to the author with an invitation that he submit, for the use of the reviewer, a detailed commentary on the review-provided he feels comment is needed and provided it would be furnished within a stated brief time. (2) CP would transmit this comment to the reviewer. (3) When the reviewer replies to CP, CP would inform the author about the changes made or report that the reviewer wishes the review to remain unchanged, and CP would invite the author if he could comply with certain strict limits of space and time-to send an addendum to be printed with the review of

Many reviews would need no comments. On the others, the procedure would, admittedly, delay reviews by a month or two. However, since the interval before publication of reviews in *CP* has typically been about 10 to 12 months (cf. *CP*'s Annual Report of March 1961), the disadvantages of such extra time might well be outweighed by the greater accuracy of reviews and by the heightened liveliness and usefulness of *CP*'s pages.

Let us now illustrate the possibilities and problems by referring to Jensen's review, citing first a small detail to illustrate the sort of questions which might most easily be cleared up.

In his review, Jensen said "the authors have drawn upon innumerable, personal

anecdotes, case histories, records of psychotherapy, student autobiographies, descriptive anthropology, and works of fiction, in one instance quoting a passage of 10,000 words from a novel by Lillian Smith." Disregarding the "innumerable," which may not have been intended as a modifier of more than the first item, we wrote to Jensen to ask why he assumed that The Journey by Lillian Smith was a novel rather than a record of her knowledge of actual persons, and asking him, even aside from this, what other "work of fiction" justified his use of the plural. He replied that he had not known about the nature of this particular book by Lillian Smith and stated: "Because of the great amount of case material, biographical selections, etc., I must have been left with the impression that there was more than one quotation from some work of fiction. I'm sorry for the mistake."

Points which could be cleared up as easily as this would generally be small details. Even so, reviews would be better without such flaws.

The most interesting and important questions, however, are not those where some single clear-cut datum could resolve the question. Instead, they are points where a reviewer makes a summarizing statement drawn from consideration of many different sections of the book under review. On these, however, not only is there some increasing risk that the reviewer may misperceive the material, but also the likelihood is that the author's communication to the reviewer would not lead the reviewer to make any essential change in his descriptive statements, even when change would have been appropriate.

We believe the following illustrates this kind of contrariety. Jensen's review had said that the authors not only neglected material from experimental research, but also that "occasionally they even make slighting remarks about experimentation and laboratory investigation as being 'colorless and neutral.' "We asked Jensen whether he could document this statement, indicating pages where we had used such expressions as "colorless" to characterize the kinds of conscious experience involved in traditional experiments in perception. But, we said, we nowhere had described laboratory investigation in such terms-on the contrary, our statements had been such as this: "Even if we take some of the simplest of traditional experimental materials from perceptual research, . . . we find a number of principles that possess fundamental importance for the entire field of personality . . ." (p. 195; see similarly pp. 133, 152, 164, 180, 182).

Jensen replied: "As for slighting laboratory experimentation, rather than saying that you made slighting remarks, perhaps I should have said that you at times unfairly put this approach in an unfavorable light. When most of the laboratory work in perception and learning has not been directed at the problems you consider important for personality, it is unfair criticism of the laboratory approach to point out that laboratory studies have nothing to say about such complex behavior as 'the richness and grandeur and heroism that men reveal in real life'" (p. 197).

This reply too, it seems to us, illustrates the need for something more than just a communication from the author to the reviewer. In the first place, it is to be noted that the quotation cited by Jensen indicates merely that we used the phrase, "the richness and. . . ." Neither it nor the surrounding material, it seems to us, gives support to Jensen's crediting us with statements tending to "point out that laboratory studies have nothing to say about. . . . " Indeed, the quotation used by Jensen came at the end of Chapter VI, the whole content of which was an attempt to show that, even though "profoundly significant human experience" cannot be brought into the laboratory, the laboratory is needed as "the means whereby a better understanding could be achieved of something massive and important" (p. 180). The chapter presented the case from Lillian Smith, mentioned above, but then used it to argue that there is a whole series of respects in which a much better understanding of such a complex case can be secured by laboratory research on much simpler phenomena. As we said in the last paragraph before the chapter summary: "In this chapter, we stayed mostly with those traditional materials-with things that appear to be trivialities of experience. We hope we have demonstrated that, even with these, there are some laws which are indispensable for understanding even the most complex and profound of human experience." So, as we see it, the question that Jensen should have raised is not whether it was "unfair" of us to beat our wives, but why we were acting so devotedly!

We do not mean that Jensen's review was entirely wrong. If we could have written a postscript, we ought to have included in it an acceptance of Jensen's criticism that the book not only avoids using the factor-analytic type of study of personality but also reports far too little of the other systematic research by academic psychologists on personality as such. Furthermore, we ought not to have protested if, in addition to saying that we had

the "purpose of presenting personality in a richly human perspective," he also had said that "they try to serve this objective in a peculiar way, trying for the most part to show how this richly human material can be understood in terms of abstract principles drawn from general experimental psychology, observations of behavior of animals, and observations of relatively simple real-life phenomena."

This last quotation is not, however, the picture that Jensen's review presented. He contrasted two main approaches. One, he said, seeks to understand personality in abstract, theoretical, nomothetic terms. The other "seeks a kind of understanding called Verstehen—an intuitive, holistic, empathic, appreciative way of viewing and interpreting phenomena in terms of one's own feeling states." In the case of our book, he said, "both the strengths and weaknesses of their work are largely a result of this holistic, idiographic, Verstehen-type approach."

Obviously, it would be impossible for an author to provide sufficient evidence in a brief postscript to challenge adequately any such sweeping characterization; but an author could, at least, raise questions as by pointing to internal contradictions within a review. In our own instance, for example, it seems relevant to note that Jensen described the book as indebted to Köhler, Tolman, Adler, Horney, Sullivan, Rogers, George Kelly, and Freud, and spoke of it as "the first introductory textbook in the personality field having a field-theoretical orientation"!

The real issue between Jensen and ourselves, therefore, seems to be, not whether it is important to work out abstract principles regarding personality, but whether the best data are those that factor-analysis uses and whether the best methodology is that of statistical analysis, or whether, on the other hand, personality is so largely situational (both because personality habits are relatively limited things and because habits that operate in real situations do not operate in questionnaire-answering situations and other situations adapted for mass testing) that a different method of "analyzing factors" must be used.

On such basic points, authors and reviewers often could not agree. Nevertheless it would seem like a good experiment to have CP try to bring the differences between authors and reviewers, where there are such, into clearer focus.

ROBERT LEEPER University of Oregon PETER MADISON Swarthmore College

KOCH'S VOLUME 3

It is hard to see why Professor Gough took so long to say that he did not like the editorial methods of Koch's volume 3 (Psychology: A Study of a Science, Vol. 3: Formulations of the Person and the Social Context; CP, May 1961, 6, 154-156).

While some of the criticisms concerning the volume's structure appear justified, one would hope for a response to so monumental an effort more penetrating than ". . . its participants . . . with minor exceptions have not said anything new or different here." In the first place, this is clearly not true, to wit, the creative new syntheses by Rapaport, Rogers, and Katz and Stotland. In the second place, it avoids anything resembling a substantive analysis of the views presented, their relation to the mainstreams of current thinking and research, and the meaning their current status has in relation to the history and future of psychology. This omission is equally serious in regard to Koch's brilliant evaluation of the three volumes of Study I (Epilogue). This commentary, ranging from the re-analysis of S and R to the revivified concern with analysis of experience, is, in my opinion, must reading for every psychologist who would hope for a refreshing insight into the recent history and current status of systematic psychologies.

I would hope that CP's editor would not permit this volume to pass by without an additional review_concerning its substance. ALLEN E. BERGIN Wisconsin Psychiatric Institute

FRUSTRATION BUT NO REMEDY

Let me add my voice to what appears to be a rising chorus of dissatisfaction with many of the reviews in *CP*. Without any objective evidence for my statement, it is my impression that the number of inadequate and unsatisfactory reviews is increasing. Perhaps the increasing number of letters in On the Other Hand is evidence of a sort but there are no doubt other factors than the feeling of dissatisfaction which contribute to this increase. [The letters began to be many and significant in 1959; the peak year was 1960; there is a slight decrease so far in 1961.—Ed.]

I agree that On the Other Hand is an outlet for the airing of disagreement, but it seems to me it is no excuse for inadequate or inaccurate reviews. An author should not have to correct or defend himself against misstatements of fact. Nor should readers of reviews have to complain that the review told them nothing about the book. In a situation where the review in *CP* may be the only, or the ma-

jor, review of a book, it seems to me that CP has a responsibility to the profession to see that its reviews are adequate and fair.

There seems to be a trend for reviewers to use a review to air their own biases or opinions, or to introduce comments bearing little relationship to the book under review. The most recent example is in Gustad's review of Brammer and Shostrom and Cottle and Downie (CP, Aug. 1961, 6, 269–271). While it is interesting to read these comments, is CP the place for their publication?

An analysis of the causes and possible remedies for the situation is beyond the intentions of this letter. The selection of reviewers obviously is an important factor. I am aware of the difficulties of obtaining reviewers, including those pointed out in *CP* Speaks (*CP*, Nov. 1959, 4, 355).

There must be some way, however, to avoid having so many reviewers who utilize a review as a platform for pointing out, or implying, that they could write a better book than the one under review if they wanted to. In other words, there seems to be too little humility in the reviewers, particularly those who have never written a book. Is it because they are young, inexperienced, immature—even though, obviously, bright?

Isn't there some way to obtain reviewers who are interested in the book to be reviewed, who have a desire to review it, whose reaction is spontaneous, rather than the labored product of an assignment, so easily used as an opportunity for raising one's ego by deflating that of an author?

C. H. PATTERSON University of Illinois

SENSITIVITY SOLICITED

For shame, sir [that means CP], for shame!

In your introductory biographical note to the review of B. D. Lewin's and Helen Ross' Psychoanalytic Education in the United States (CP, Aug. 1961, 6, 265f.), you refer to Miss Ross as a "lay analyst." Your use of this unfortunate term plays right into the hands of those benighted medical analysts who look upon the practice of psychoanalysis as being part of their medical bailiwick and who, on occasion, condescendingly permit a nonmedical 'outsider' to practice it in the same manner in which a member of the clergy might occasionally permit a lay member of his congregation to preach a sermon.

If the term was supplied to you by Miss Ross herself, or by some other member of the American Psychoanalytic Association. and was used to indicate that she is a 'lay analyst' in the eyes of that group, then you should have indicated this. If the term is of your choosing or of your reviewer's, then you have been insensitive to the feelings of many of your fellow psychologists.

Let this term be henceforth banished from all psychological publications.

New York City

BOOKS RECEIVED

Benda, C. E. The image of love: modern trends in psychiatric thinking. New York: Free Press of Glencoe, 1961. Pp. xviii + 206. \$5.00.

Brand, Jeanne L. Private support for mental health: a study of the support, by foundations and other private national granting agencies, for mental health and related disciplines. Washington, D. C.: Public Health Service, National Institutes of Health, U. S. Department of Health, Education and Welfare, 1961 (distributed by Superintendent of Documents, U. S. Government Printing Office). Pp. vi + 45, \$.35.

CHOISY, MARYSE. Psychoanalysis of the prostitute. New York: Philosophical Library, 1961. Pp. 138. \$4.75.

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EDUCATIONAL RECORDS BUREAU. 1961
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79.) New York: Educational Records
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HAYNES, RENÉE. The hidden springs: an enquiry into extra-sensory perception. New York: Devin-Adair, 1961. Pp. 264, \$5.00.

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JOHNSON, W. F., BUFORD STEFFERE, & R. A.

EDELFELT. Pupil personnel and guidance services. New York: McGraw-Hill, 1961. Pp. xii + 407, \$6.50.

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PALMER, C. E. Speech and hearing problems: a guide for teachers and parents. Springfield, Ill.: Charles C Thomas, 1961, Pp. xiv + 137, \$5.50.

RODGER, R. S. Statistical reasoning in psychology: an introduction and guide. London: University Tutorial Press, 1961. Pp. viii + 204, 15s.

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Stein, M. I. (Ed.). Contemporary psychotherapies. New York: Free Press of Glencoe, 1961. Pp. x + 386. \$7.50.

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WESTMAN, H. The springs of creativity. New York: Atheneum, 1961. Pp. xiv + 269, 86,95.



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